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THE

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REVIEW

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Analytical and Critical Reviews.

I.—The Treatment of Phthisis.¹

THE claims sometimes put forward on behalf of therapeutics would, if admitted, make the systematic study of pathology well nigh impossible. The due observation of disease undoubtedly depends upon the fact that, strictly speaking, disease itself is for the most part incurable. It is recovered from, but it is rarely stopped by outside interference. If it were otherwise, and appliances had a real efficacy to control or arrest morbid action, the humanity which compelled their employment, would at the same time conceal the phenomena to be observed, as is in a measure the case with ague and smallpox. The study of the natural history of disease would thus be confined to savage communities, or at least dependent on the rare opportunity of inspecting the bodies of those whom some hard fate had suffered to perish by a natural death.

The morbid anatomist is little disturbed by such considerations. To him the course of life presents itself as one of progressive and orderly deterioration where, although the manner or the rate may change, the direction is always the same, and steps once taken are never retraced. In what passes for cure he discovers only a rough mending, which can never restore the integrity of the part, while in those secondary vital changes

¹ 1. Dr. WILLIAMS (chaps. xxy-vi), *Pulmonary Consumption*.

2. Dr. HERMANN WEBER, *On the Treatment of Phthisis by Prolonged Residence in elevated Regions*. 'Med.-Chir. Trans.,' vol. lii.

3. Dr. THEODORE WILLIAMS, *Lettsomian Lectures on the Influence of Climate in the Treatment of Pulmonary Consumption*.

4. Dr. J. F. CHURCHILL, *Consumption and the Hypophosphites*.

commonly regarded as compensative, he sees no more than a blind and inadequate provision which, though it may serve its turn for a while, adds a new element of danger and extends the area of actual disease.

If this view of the matter needs modification and exception, yet even as stated, it does not shut out the function of therapeutics. To the eye of the mere anatomist the course of disease gives evidence of deviation and prospect of pause. Possessed of post-mortem evidence that morbid processes are actually arrested here and there it remains for clinical observation to discover the precise conditions under which the arrest takes place, to ascertain, especially, whether this occurs as the result of accident, or may by any be means solicited by a particular method of treatment. The expectation of therapeutics is thus founded on the sure basis of morbid anatomy. The observation that disease stops gives ground for the hope that we may be able to stop it. Whether or not such hope is fallacious must appear upon further and independent inquiry.

Pulmonary consumption in its most familiar form gives apt illustration of these principles. The lung of chronic phthisis represents a world of change and variety. Destruction and repair are to be seen side by side, so that a single spot will include softened lung tissue in rapid progress of disintegration, and tough fibroid material designed to limit the area of mischief in some particular direction. Death in such cases is not the end of a continued process of decline, but rather the close of an unequal contest between decay and conservation, where up to the last days of life reparative processes may be still at work. And, moreover, when the history of the patient happens to be known, it is often possible after death to connect some particular portion of healed lung with some period and incident of the individual life, in such manner as to make it probable that the arrest of disease in that spot has been directly due to our own interference and the measures taken at the time with that object.

It is thus by a perfectly legitimate train of observation that the practical physician in his study of chronic phthisis sets himself to inquire, whether there are means within his reach of exciting or promoting those processes of healing which it is clearly within the plan of nature to set up. To such repair he may well conceive certain modes of life and conduct must be favorable and others prejudicial. By experiment and comparison he may hope to discover, and even to demonstrate, what happy combination of circumstances shall best hinder the progress of a disease, which is naturally subject to transitions and susceptible of cure.

It is to be observed, however, that those anatomical characters which in the case of chronic phthisis give ground for a reasonable hope that human contrivance may come to the aid of nature in arresting degenerative changes are all absent in the acute form of the disease. The intervention of fibroid material, for instance, which occurs in chronic phthisis is the means by which the mischief is more or less completely circumscribed, and eventually, by a process of contraction and cicatrization, rendered obsolete. The expectation of cure which is founded on this appearance takes the definite shape of being able, by some means or other, to promote and extend a curative process which is seen already at work. But acute phthisis possesses no anatomical character on which to build a similar hope. Its steps are traceable, indeed, like those of the other, but they are all in one direction. Though one portion may lag behind the other there is a common destruction everywhere.

Morbid anatomy supplies here no material for the service of therapeutics. Regarding the method of the destruction it is hard to conceive of any anatomical means by which its progress might be arrested. Regarding the wide extent of the mischief, it is difficult to believe that any event whatever, short of actual restitution of spoilt tissue, can suffice to meet the needs of the case. If we may not say that acute phthisis is from its nature beyond our reach from the very first, we may at least say so as soon as it becomes recognisable with certainty. So obscure, indeed, as every one knows, are the earliest signs of this affection that, even supposing it within the scope of sober therapeutics to undertake the cure of it, it would be impossible to demonstrate that such cure had actually been effected. It will always be more probable that the diagnosis was wrong than that the treatment was effectual.

Such antecedent considerations derived from our anatomical knowledge of the disease (which in certain directions, at least, is now sufficiently precise) are not to be lost sight of on approaching the subject of treatment. Prior to all analysis there is that in the nature of the case which imperatively claims to govern the judgment. Of the pretensions of therapeutics with reference to phthisis, which we are now to consider, there is something we are prepared to accept, something to regard as antecedently improbable, and something to dismiss at the outset as lying beyond the range of possible proof.

With this much of necessary preface we propose to review the present state of medical opinion on the subject of the treatment of phthisis. Avoiding all theories or expression of personal views, we would confine the reader's attention to the remedies

actually in use, the promises held out on their behalf, and the existing evidence upon which these promises depend.

Speaking at present of chronic phthisis, and making no distinction—as, indeed, from this point of view there is none to be made—between the tubercular and the non-tubercular, we may roughly divide the prevailing methods of treatment into three classes. First, there is the method of those who, regarding phthisis as beyond the reach of prevention or cure by direct measures of their own, nevertheless recognise the general service, and even the special use, of dietetic and conduct rules. These believe that the phthisical patient, like the rest of mankind, is accessible to the benefit of pure air, wholesome food and a tranquil mode of life. They would even maintain that, from the nature of his disease and from a considerable mass of evidence, it is highly probable that pure air is of special benefit to the consumptive. They deny, however, that there is any known means of directly promoting those curative lung processes which, as they assert, will occur or not according to the genius and character of the particular case. Secondly, there are those, mistrusting like the first medicinal cures, yet regarding climate as having a control over phthisis beyond that which is conceded to it in the universal belief that men thrive better in fair weather than in foul. These, or the majority of them, do not assert that the climate which is curative of phthisis can be indicated as this or that, according to the form of the disease; they are, in fact, in active search for such information and perplexed between statistics and theories; they hold, however, in general terms, that phthisis, even in an advanced stage, may often be cured by an appropriate climate, admitting that the procuring of such climate in individual cases must be the result of experiment. They would further claim for this same agent prevention as well as cure, and can adduce instances of ominous catarrh and wasting with suspicious physical signs disappearing under such treatment as examples in point. The third class may be shortly described as including those who believe, some more, some less, but all implicitly, in the curative power of drugs.

It is subject for remark that in this category the third class, numerically at least, should be by very much the smallest and, whether rightly or wrongly, the least influential. That change in medical opinion, affecting not one disease merely, but the fundamental principles of treatment, is to be described rather as revolution than reform. It is nowhere better illustrated than in the case of phthisis. A living writer, one who has placed on record a rich store of material as to the results of treatment, relates that he had recourse in his own early practice to mer-

curials and moderate bleeding for consumption and that even now he is not prepared wholly to abandon them :

"I apprehend," says Dr. Williams, "that most practitioners in this country are agreed in considering that consumption should be generally treated on a tonic and sustaining plan, and that the nourishment and strength of the system should be supported by varied tonics and cod-liver oil, as well as by the most nutritive articles of diet. But when the disease is ushered in with symptoms of acute bronchitis or pneumonia, with its attendant fever and recently disordered secretions, it is obvious that such treatment is wholly unsuited for the occasion, and that remedies of the mild antiphlogistic kind, such as salines, with or without antimony, blisters, and cataplasms, and sometimes even moderate leeching or cupping, will give much relief, and will prepare the patient for the safe administration of the sustaining class of remedies."

And again :

"In case of active inflammation it answers well to withhold or withdraw the stronger stimulants and tonics, and for a time—it may be a few days only—to substitute cooling and soothing remedies, and, more rarely, local depletion. But this discipline, which is exceptional, should as soon as possible be replaced by what may be called the antiphthical treatment," &c. &c.

Comparing these apologetic phrases with the words of the same writer thirty years ago, they may fitly be described, in their reserve and exception, as the treatment of a transition period. There is the tone of indecision and invitation to parley that commonly precedes unconditional surrender.

The moderate and restrained practice of to-day, whether in phthisis or any other affection, is in fact less the result of study directed to the particular disease than of a revolution of thought and opinion which from a higher region has penetrated every department of medical practice. It is not that anything new has been discovered, but that much that is old has been discarded. A lessened regard for authority, a more rigorous scrutiny of the grounds of belief, an attitude of scepticism, which, whether good or bad, is not confined to medicine, has led not only to the discredit of drugs but to the fuller use of those natural means of health restoration which are almost instinctively recognised. It is part of the regular business of therapeutists to be for ever seeking after and asserting new remedies, while as the demand for proof becomes more peremptory it is easier to displace any prevailing system of drug treatment than to find a footing for a fresh one. As a natural consequence the tendency strengthens in the enforced abandonment of classical methods of cure to have recourse to means which appeal for approval to universal experience. Hence it hap-

pens, independently of any special evidence, that a sceptical age revolting against drugs and phlebotomy recognises anew the virtues of pure air and good food and fair living.

It may appear, indeed, to the student of the present day that much which is now written in reference to the treatment of phthisis might be taken for granted without specific statement. Yet the actual fact is that directions which now appear obvious and almost self-evident have not been reached at once or without costly experience. It is no self-evident proposition that the subject of active disease is best dealt with by the very same means which would best promote his ordinary health. Assuming it to be true it is a discovery, and a discovery as every student of medical literature knows, of which in all the strife of systems there was no hint or suggestion until lately. When Dr. Williams announces that "the treatment of consumption should be of a sustaining and invigorating character; that it should include the most nutritious food aided by a judicious use of stimulants and of medicinal tonics with such varied and moderate exercise as the strength will bear, with bright sunshine, agreeable scenery and cheerful society," he is so far from stating what we might have known already that there are many experienced physicians (and in places, as we have seen, Dr. Williams himself would seem to be of the number) who regard such counsel as too bold and unreserved.

With the acceptance of such advice, be it right or wrong, the consumptive patient, liberated at once from a host of restrictions, is committed simply to that mode of life which all would choose whether ill or well. It remains to be seen whether there yet linger any special directions applicable to his special case, whether, that is, *the kind* of climate or of stimulant, or of medicinal tonic is to be prescribed for him by authority, or whether he may select these, as he does his society, in accordance with his individual tastes.

Upon the subject of treatment (be the affection under discussion what it may) the reader soon learns to distinguish between that which is direct and explicit and that which lawyers would describe as "common form" as applying equally to all illness whatever. Take what disease you will and the section devoted to treatment will be found to be made up of these two elements, the special and the general. There is that which is, or which claims to be, of particular service to the disease in question, and there is that which it is always thought proper to rehearse as generally necessary precept. There are diseases enough where from our complete ignorance "general principles" occupy almost the whole ground, and there are some—there is at least one, where the remedy is so direct and notorious that

much supplementary comment might seem superfluous ; but in all descriptions whatever treatment must be discussed under the two headings. So habituated are we to this rule that not only do perfectly obvious statements in regard to diet and regimen pass current as useful information, but any writer who should neglect to pay his service in both kinds would be accounted bald and incomplete and forfeit his reputation as a "practical physician."

If we apply this mode of division to the subject before us, it will at once appear that "general principles" occupy by very much the larger portion of the whole space devoted to the treatment of phthisis. Excepting, of course, cod liver oil, the medicines suitable for the disease, whether tonics or stimulants, would seem to be such as suit the general condition of the patient and are not suggested by any distinctive feature of the particular affection from which he is suffering. Thus, directions for administering iron or quinine or mineral acids proceed upon general grounds, they are to be commended in certain conditions of the body whether of phthisical persons or others, and there is no proof or allegation of their being of better or other service owing to the existence of that particular disease. It is the same with the intercurrent affections to which phthisical patients are liable. Dr. Williams' suggestions, for example, as to the treatment of indigestion or of bilious attacks do not more strictly apply to the consumptive than to others who exhibit the same symptoms and are of the same general tone of health. In a word, while the dietetic and regimenal treatment of phthisis in the present day is precisely that which modern views would commend as suitable for the well-being of everybody, its medicinal treatment is no other than that which would be needed for a set of symptoms common to many other diseases besides phthisis. Of any special mode of life suitable to the consumptive as such, or any special drug treatment applicable to his occasional ailments as being his, although not without theories, we have as a matter of fact, from writers of the school of Dr. Williams, no particular information.

There is, indeed, as we have said, cod liver oil, "a remedy," according to Dr. Williams, "which may truly be said to have so much altered the prospects of the consumptive as to give hope of cure in not a few and of much prolonged life in by far the greater number. *Here is the remedy,*" he adds (to repeat his own italics), "the only one worthy of the name which, if carefully and faithfully used, may arrest and cure the disease *and is pretty sure to retard it* (the italics are now ours) and prolong life more than any other known means." Speaking as we desire to do of the naked results, and not of the theories of

treatment, it would be out of place to follow Dr. Williams in his explanation of the mode of action of cod liver oil "in promoting the healthy growth of the protoplasm and counteracting the morbid tendency to the proliferation of the decaying cells of various cacoplastic and aplastic matters." That may be all very true, but for our present purpose we abide by the plainer language. Cod liver oil "*is pretty sure to retard*" phthisis. Will that sentence live? Coming from revered authority, does it embody the truth in regard to this agent, or will it, like every other sentence ever uttered expressive of a distinct opinion as to the action of a drug, be read with modification after a certain interval? There is at least another and a lower estimate of the value of cod liver oil in chronic phthisis. It is that of those who admitting its use, are in the habit of seeing it fail entirely to make any impression whatever upon the disease, even when it is taken readily; who find moreover and not seldom, that under whatever disguise, it is sometimes quite impossible to administer it; who can even point to its occasional evil effects where it is violently persisted in regardless of the remonstrance of the patient. Cod liver oil is most assuredly a remedy not to be despised in an affection so bare of remedies as pulmonary consumption. It is to be commended with every mode of persuasion which may be derived from the numerous instances where wasting has stopped and cough become less harassing by its means; nay more, it is quite justifiable to point the patient's attention to cases of that kind rather than to the much larger class to whom cod liver oil was of no service whatever, or, at the most, was only of slight and temporary use. Dr. Walshe, more cautious in his expressions, yet believes that the oil "indubitably possesses the power of prolonging phthisical life," and that it "induces improvement more rapidly and effectually than any other known agent;" but he regards the permanence of its good effect and its power of *curing* the disease, as undetermined, and implies that it sometimes fails altogether.

And surely in a remedy like this, to which at the present day almost every one convicted or suspected of phthisis immediately flies, it implies a somewhat exaggerated deference to authority to hesitate before asserting that cod liver oil is often quite useless. Is there a single physician who cannot illustrate as much at any moment from his own practice? In the very large body of evidence that we have at present in reference to this remedy, the difficulty would rather be to show that cod liver oil was of more benefit in phthisis than in some other diseases accompanied by general emaciation. Actual observation witnesses to its utility in improving nutrition in wasted subjects, whether phthisis be present or not; it discovers besides that the agent

is most serviceable in early life, and in that class of affections loosely held together under the term scrofulous; but it fails to make out a case for cod liver oil as a special remedy for pulmonary phthisis. Dr. Williams, it is true, finds that the average duration of life in phthisis has quadrupled in his experience. Dr. Walshe expects to find by and by evidence in the Registrar-General's Returns of some diminution in the annual mortality from phthisis directly due to the virtues of the oil. It would be hazardous to press either the fact or the expectation into active service. If we are to begin to count those saved by cod liver oil we must reckon as well those destroyed by "mercurials and moderate bleedings." If we are to congratulate our age upon the happy results achieved by the modern treatment of phthisis, including sunshine, rest, fresh air and suitable climate, it is obviously improper to place all the life saved to the account of cod liver oil.

For it appears as we read on that there is a mode of encountering phthisis which claims to be more effectual than any other for its permanent cure. It is *climate*. If not altogether modern, it has at least grown and expanded with modern habits of travel and improved means of transit. It is, indeed, not a little suggestive to trace the gradual development of this treatment. The consumptive, finding himself better by change of sky, has ventured further and further afield, and the doctor, so to speak, has followed him. Restricted at first to warm and sheltered places, the patient presently finds by trial that the keen air of the mountain and the bracing breezes of the north, in spite of his failing health, have not lost their old power of exhilaration. Upon that discovery it is not difficult (in practical medicine it is never difficult) to adjust scientific principles to observed facts, and to point to the special conditions of elevated places or of low temperature as calculated to be of service in the circumstances of the case.

Of the individual climates of the many lands at present resorted to by consumptive patients we do not propose to speak. The subject has been lately discussed in this Journal. But of the *kind* of evidence for climate there can be no better or fairer illustration than that adduced in favour of elevated regions by Dr. Hermann Weber in his communication to the Medical and Chirurgical Society. The seventeen cases there quoted are, as he observes, too few to permit of a decided inference. The agencies concerned are very complex. For the good result Dr. Weber leaves it to the reader to choose between increased respiratory movement, larger proportion of ozone, dryness of soil and scantiness of population. Nor would he restrict his view to meteorological conditions alone; he suggests that

the infrequency on the mountains of intercurrent catarrhal attacks may be due to the fact that *the sight of snow and ice* reminds the invalid of the necessity of warm clothing. At the same time, the elevated region offers less danger from unsuitable exposure, because less temptation to incur it. The invalid of the Engadin has the advantage over his brother of the Riviera in escaping the perils of evening parties. "I wish by no means to be understood," adds this writer, "as if I were under all circumstances in favour of high-level in opposition to low-level health resorts."

It is to be observed, moreover, as the title of Dr. Weber's paper indicates, that the benefit held out to the consumptive from mountain air is to follow a "prolonged residence." It is difficult to institute a comparison between places made use of for winter refuge, like Nice and Cairo, and those chosen for lengthened or permanent stay. Thus, for example, a patient of Dr. Weber's (Case 3) improves during successive winters at Cairo, and at Cannes and Nice, but gets worse repeatedly during the intervals spent in London. He is then sent for eight months to the Peruvian Andes at an elevation of near 10,000 feet, and next, after a short relapse at New Orleans, to the tableland of Mexico, "where, to his own feeling, he entirely recovered his health."

In another instance (Case 6), after repeated attempts to resume his occupation, and experiencing temporary relief on several occasions by short residences in various places, the patient is at length induced to spend three entire years on high ground, and becomes permanently cured. There are other examples of the same kind. Dr. Weber's evidence in favour of elevated regions is striking and important, but it applies only to long residence. It cannot with propriety be compared with the customary wintering abroad of so many of our English consumptives. The cure follows (or may be hoped for) after a period of years; but by what method it is accomplished, whether by the rarer air, or the drier ground, or the sight of snow, or the freedom from care, Dr. Weber, with commendable caution, declines an opinion. Here, then, we have certain encouraging results, but they are within the reach of but few, while an exile so long and so peculiar is likely to be acceptable to still fewer. The misfortune is the greater that no inference can be drawn from such facts; that it is impossible, with all our knowledge of phthisis, and all our knowledge of meteorology, to seize upon the particular element of good. We have the observation that certain spots of the habitable globe are often beneficial in phthisis; we are unable to utilise or apply

the fact for the good of that great multitude who are unable to use it literally.

Something of the kind has indeed been attempted by Dr. Theodore Williams, who, with characteristic industry, undertakes the analysis of 251 patients and ventures to draw conclusions therefrom, which, we make bold to say, no single reader will dream of accepting upon the evidence. Dr. Theodore Williams manipulates his facts with skill and patience, but it must be obvious even to himself that they are insufficient for his purpose. Twelve patients spend twenty four winters at Mentone; two are much improved, three are improved, two remain stationary, and five get worse. Howbeit, eighteen patients in forty-five winters took sea voyages to Australia, to America, to India, to the Cape, to the West Indies; they went in some sort of ship somewhere; seven were much improved, nine improved, one remained stationary, and one got worse. The very same number, eighteen, went to Rome, and in twenty-two winters six got worse, and only four were much improved. Upon facts of this sort, dealing with the total number of patients just stated, we are invited to the conclusion that sea voyages come first, very dry climates second, dry South of Europe climates third, moist climates like Rome fourth, Madeira and the Atlantic fifth, and Pau last.

In this "tangled web of figures" it is improbable that any should be ensnared unless it be he that spins it. In constructing from such slender material a graduated rate of this kind the service of statistics is reduced to an absurdity. If seven patients out of eighteen get much better at sea during forty-five winters, and only sixteen out of forty-three get better in seventy-six winters at Pau, how much better is the sea than Pau as a refuge for consumptives? It would appear, indeed, that these conclusions have been sharply contested by the medical residents of such "health resorts" as happen to figure at the bottom of the list. Dr. Theodore Williams, however, sticks to his point, quoting "the old Roman," *Fiat justitia ruat cælum*. The quotation is hardly more fortunate than the figures; neither will endure inspection. With the form and semblance of double rule of three we have a sum which is in fact insoluble, while the line which suggests Cicero or Seneca is certainly by no "old Roman," and not to be traced beyond Erasmus.

It is fair to say that Dr. Theodore Williams, with all his boldness, "would not decree exile" to those who "cannot make themselves happy," nor would he send patients even on sea voyages except with caution and after "much consultation." His final recommendations indeed are not derived from his

“tangled web of figures,” but from common sense and observation. Those who are getting better or who are very bad are best, he thinks, at home. Those with whom the disease is quiescent may try another climate if they please, and if foreign cookery is to their liking; those who have been most confined indoors are likely to benefit most by out of doors. The air of mountains promotes the free expansion of the lungs. If such expansion be desirable—no one can say for certain that it is, but Dr. Theodore Williams believes it to be so—let it by all means be resorted to in “*suitable cases*.”

It must be borne in mind that in this matter of climate the number of those who are entitled to speak with authority is not large. It is unlike the experience to be gained as to the effect of drugs or cod liver oil, where, however complicated the problem, hospital practice opens to all an inexhaustible field of experiment and observation. The climate cure is open to a certain class only—the class of private patients. It is improbable that experiments in this direction will ever be carried out in public on a large scale. We have to listen, therefore, to the conclusions of those whose lot it has been during a long period to advise the well-to-do classes and who have been at the pains to note the result.

For this reason no less than for his scrupulous accuracy and logical precision there is no single authority whom we should hear upon this subject with more respect and confidence than Dr. Walshe. His opinion may be gleaned from a very few quotations of the last edition of his well-known work on diseases of the lungs:—

“Whether there be any climate specially fitted for the consumptive is yet matter for inquiry.” “We cannot announce *à priori* the influence which any one climate will exercise upon the inhabitants of another, though the meteorology of both regions be perfectly worked out according to the standard of existing physics. The physiological influence of all varieties of change of climate must be observed to be known and understood.” “Those climes which furnish from time to time the most striking examples of arrest of phthisis are glaringly deficient in one important element (*i. e.* steadiness of temperature) of theoretical success.” “All climates which ought on the orthodox theory of the hour to have agreed have disagreed, and all climates which ought to have disagreed have agreed.” “There is something in the pathological idiosyncrasy of individual cases that dominates climatic influences, and according to its nature favours, weakens, or nullifies these.”

Upon such statements as these, which might well surprise those to whom in Dr. Theodore Williams’ phrase exile is decreed, we may conceive the advocates of climate cure aban-

doing the search after a locality marked out by its meteorology as theoretically good or bad for phthisis, yet clinging still to the belief that countries must be bad where phthisis is actually bred and those good where, as a native product, it is almost unknown. Yet this position is as untenable as the other. Why should it follow that a country free, or almost free, from a particular disease should offer a suitable asylum for those who have already contracted it? A country may fail to produce a disease, while at the same time it is eminently unfriendly to it when imported. "Who would think," says Dr. Walshe, "of submitting an English phthisical sufferer to the tender mercies of the climates of the Orkneys, Iceland, or Siberia, even though he were reminded that, as is the fact, in those latitudes indigenous phthisis was little known?" Conversely a country may be beneficial to imported consumptives, although it does not spare its own offspring. We have seen what is to be said of elevated districts, yet the watch-makers of the Jura, we are told, suffer much from phthisis. "Probably," says the author we have been quoting, "this earth offers few known spots more favorable to the tuberculized British patient than Nubia. Yet the native Nubians on their own soil are occasionally destroyed by phthisis." And again, "while Southern Devon is a favorable resort for phthisis of Londoners, it seems a very hotbed of the disease for its own native population."

It would be beyond our present purpose to consider in detail, as bearing upon the subject of treatment, the valuable series of observations by Dr. Buchanan and by Dr. Bowditch of Boston, showing conclusively that drying of the soil by effective drainage is followed by a diminution in the phthisis death-rate. While the fact is accepted extreme caution must be exercised in the use that is made of it. Drainage has other objects and results than the drying of the soil. A lessened death-rate from phthisis does not prove that dampness of soil *per se* is a cause of the disease. Against the effect of drying we must take the effect of wetting. If as regards phthisis there is evidence of the good of certain dry soils and of the use of drying soils that are wet, there is evidence not less distinct to the direct contrary. Lincolnshire, exceptionally malarious, has a low phthisis mortality; in marshy places pervaded with ague phthisis hardly occurs until the district is drained, when the ague departs and phthisis comes in its place. If dry elevated places are notorious as curative of imported phthisis, the marshes are not less notorious, and in both situations (though probably in the swamps most conspicuously) the disease is rare as a native product. We have to reconcile the facts, not to make our own

selection from amongst them in support of preconceived opinions. Dr. Williams believes that warmth is likely to hasten the course of phthisis by its tendency to "blight or corrupt portions of the bioplasm." Lehmann, upon chemical considerations not less valid, insists that a moist warm air is as good as cod liver oil and other medicines. There are places on the earth's surface to serve the turn of both observers. It is the same with alcohol, which is adduced, and perhaps with reason, both as a cause and as a cure of phthisis. There is nothing better established in the data of chronic consumption than that those following indoor sedentary occupations suffer in larger proportion than those employed out of doors. Yet here again it is only by helping ourselves to hypothetical explanations that we reach the conclusion that fresh air *per se* will either arrest or prevent consumption.

It would seem, then, there is nothing either in locality or mode of life that can be laid hold of and separated as of special service in the treatment of phthisis, nothing can be alleged in regard to climatic influence which is more definite than the general statement that the consumptive patient fares best in respect of his local disease in that place, be it mountain or plain, hot or cold, moist or dry, which is found upon actual trial to be best adapted to his general health. There is no possibility of prearrangement; it is matter of idiosyncrasy. The sum of modern experience in regard to the climate treatment of phthisis, though it has given room for infinite speculation, has not in fact established more than this, that the individual is to be considered as a whole and not in part; as having certain tastes and habits and prejudices, not as the mere possessor of damaged lungs. That climatic change offers special and exceptional hope for the consumptive as compared with other sufferers must, indeed, be freely admitted, and may even be regarded as a modern discovery; it must be admitted too that the individual so soon as he has contracted phthisis becomes more sensitive to climatic influence than heretofore. But we may not go further than this. The circumstances are too variable to admit of more precise statement. "Scientific selection" as applied to climate is but a fond phrase.

It may be urged, with truth, that the term phthisis is one of the vaguest in nosology, and that this mixing up of many distinct forms of lung disorganization renders all observation as to treatment partial and fallacious. We are not concerned to deny that there *is* a treatment and even a *remedy* for phthisis. Something may be for the good, and something for the harm of all or of most of the chronic lung diseases included under this common name; we only contend that at present this something

cannot be rehearsed in terms for any of them, and is only hit upon by actual trial.

From views like these, expressed or implied by the great body of English physicians, the change is abrupt to the confident assertions of those who would have us believe that phthisis is to be cured by drugs. It is, indeed, as we have said, an observable sign, hopeful or the reverse according to the point of view from which it is regarded, that there are but few in this country who profess so much. That these few should be impatient of the scepticism which surrounds them, and in their eagerness to obtain a hearing, less moderate in tone and more assertive than others who have nothing of their own to advance, is no more than we might expect. Nay more, if the enthusiasm of a great discovery should blind the inventor to flaws in his argument or defects of proof that too may be forgiven. Stubbornness and incredulity provoke precipitate assertions. It is to be regretted, however, that Dr. J. F. Churchill in his work upon the hypophosphites, by an error of judgment which it may not be too late to correct, should have placed himself beyond the reach of criticism. The "stæchiological" inhalations which are asserted to be curative of consumption, Dr. Churchill maintains to be his own property, the offspring of his own research, and fairly to be retained in his own hands. "Very sorrowfully and reluctantly in discharge of the obligations, which as an inventor, he owes to society," he keeps his remedy secret. It is improbable that any popular clamour will be made for its surrender. Yet never, surely, could there be a more legitimate occasion for breaking the laws of society in the general interest and resorting to actual violence than to wrest from its author so precious a boon as these "stæchiological inhalations" are said to be. In his appeal to general benevolence to assist in arresting consumption, Dr. Churchill seems to stimulate the lawlessness which elsewhere he deprecates. He concludes, on the whole, that the legislature must be forced to recognise "the right of intellectual property." He desires that this result of "his brain-work" should be protected, and that there should be "secured to discoverers in science a remuneration adequate to the value of their inventions." He takes this course, it must be added, less in his own behalf than as a matter of principle, as "a member of the noble army of inventors, whose protection is for the permanent advantage of the race." To vindicate a principle, Dr. Churchill "sorrowfully and reluctantly" declines to reveal a method by which this great national disease is to be done away. He is of opinion that what is worth so much and has cost so much should sell for something, and seems to fear that in an age of indis-

criminate benevolence, the great principle of looking after one's own interest is in danger of being forgotten.

We must decline to follow Dr. Churchill either in his autobiography or the denunciation of those who differ from his views. It may be very necessary for this writer to vindicate himself against unjust attack, but it is futile and impertinent to invite the reader to wade through pages of personal vaunting and angry defiance in search of information which in the end, from the highest motives, is formally withheld. To those who have a deep conviction of the uncertainty of drugs and of the caution and misgiving with which apparent success is to be viewed the book is, indeed, with all its boasting, a sad one. Yet these may well reflect that its evil complexion is heightened by comparison with that general tone of humility and candour which attends and befits our trusted authorities upon the same subject.

Yet, although this unfortunate work has done what could be done to discredit the hypophosphites, it is not to be forgotten that its author adduces certain evidence in favour of these drugs, and that others less partial than Dr. Churchill have found them of service. Dr. Thorowgood, for example, has published some cases of the kind, while Dr. Williams admits that "the hypophosphites of soda and lime have in his hands proved decidedly beneficial in certain cases." "They have been tried," he adds, "by Drs. Quain and Cotton at the Brompton Hospital with only negative results; but having met with several patients who distinctly ascribed their improvement to Dr. Churchill's treatment, I have thought it right to try them myself, both as a substitute for the oil and in addition to it. In the former way the results have not been generally satisfactory; the hypophosphite does not disagree, but there is no marked improvement as under the oil; and when they have been doing well under the oil the patients generally lose flesh and strength when the hypophosphite is substituted for it." . . . "How it acts is quite uncertain. I cannot say that I agree with Dr. Churchill's views on the subject even if I understand them."

For further and later testimony we may refer to the observations of Dr. Charteris, physician to the Glasgow Infirmary, who employed these drugs in thirty cases of phthisis. Of these thirteen improved, twelve did not improve, and five died. From his experience (related in the '*Lancet*' of July 29th) Dr. Charteris came to the belief that the hypophosphites checked night sweating and increased appetite. He was forced, however, "to the conclusion that if they did no good they certainly did harm, and in some measure hastened a final issue by increased fever." While admitting that patients will some-

times testify to the benefit derived from the use of these drugs (a kind of testimony, it must be added, easily enough obtained in favour of every conceivable agent administered with due ceremony and circumstance), he is "of opinion that they should by no means be used indiscriminately, and that when given, the effect should be carefully watched by daily thermometric observation." Dr. Churchill would persuade his readers that such serious falling off from the promise of his book is due to the faulty preparation of the drugs in question. It is hardly possible, it would seem, to obtain these in their purity without application to Dr. Churchill himself, and the "stæchiological inhalation" is exclusively his. It is probable that no cure can be complete without this auxiliary. We know, indeed, of other possessors of "intellectual property" besides Dr. J. F. Churchill, of one in particular whose cures are proclaimed, it is said, in every language under heaven. The particular member of "the noble army of inventors" to whom we refer, though he does not state as much, plainly implies that his pills lose half their efficacy without his ointment, nor does a patient appear in his published cases who has not made use of both.

For our own part, having employed the hypophosphites in many instances as the Pharmacopœia provides them, we incline to agree with those who, without being able to specify beforehand the form or stage of phthisis when these drugs are the most applicable, have yet found them useful, *sometimes* in improving the appetite, and thereby the strength of the patient. In common with all drugs and all measures whatsoever, they often fail altogether, and this in cases which, upon general grounds, would seem best suited for their use. The following remarks of Dr. Williams contain in epitome all that can usefully be said on the subject:

"Phthisical patients, generally with advanced disease, have long been taking cod-liver oil with phosphoric acid and some tonic, and, after improving much in flesh and other respects, come to a stand-still and get no better. Then the addition of the hypophosphite, in the dose of three or four grains to the usual mixture and oil, has produced a very marked improvement in the vigour and appetite of the patient, generally followed by increase of weight and strength. That is the empirical fact, whatever may be its *rationale*, and it has been proved too often to admit of doubt, and I am, therefore, very glad to avail myself of the hypophosphite as a supplementary aid in the treatment of phthisis."

Of how many empirical remedies has as much and more been said, and what is their common fate? Except for the grand difference between the present and the past, the virtues of

the hypophosphites are not more obvious and explicit than those of tar water; and Bishop Berkeley was probably as able and disinterested an observer as Dr. J. F. Churchill. Drugs are mortal like ourselves, and when in their turn the hypophosphites shall cease to be, the historian will seek to do even justice to all. It will then be less easy, amid contemporary praise and posthumous disparagement, to single out any particular empirical drug for special commendation, than to recognise, in the rise and fall of so many, the operation of a law of succession applicable to all alike.

With the hypophosphites, then, we exhaust the list of drugs formally proposed in legitimate medicine for the cure of consumption. There are, indeed, "adjuvant remedies" addressed to particular symptoms, and which may bring their measure of alleviation, but of these it is not our present purpose to speak. Dr. Williams, it is true, alludes to Dr. Dewar's remedy for phthisis—sulphurous acid; yet there are few who would regard this as of more than local use, serviceable chiefly in laryngeal ulceration whether tubercular or syphilitic. Other modes of inhalation directed against convulsive cough or fetid expectoration—as creasote, carbolic acid, conium, iodine—none of them, probably, without their advocates as phthisis cures, belong essentially to the same category. Of all modes of treating phthisis inhalation we believe to be one of the most uncertain.

Here, therefore, we may pause and seek the moral of our tale. It has been the work of our day to remove the restrictions which the old dogmas had set up; it is our present temper to regard with suspicion and distrust whatever it is proposed to substitute. The result is to leave the field of observation open, and render treatment less definite and less peremptory. With a far more accurate knowledge than heretofore of the pathology of phthisis, and far better evidence of the actual course and the precise mode of its cure, all special means for promoting that end have been found fallacious. We have come to concur, therefore, in effect if not explicitly, in a general policy which proceeds upon no other assumption than that the damaged lung is likely to share in the general welfare of the individual. There is no treatment of phthisis; there is a great variety of treatment, as there is a great variety of temperament, for those that suffer from phthisis.

Yet while the amplest description of phthisis treatment that can be written down as generally true for the great mass of its victims resolves itself into little more than the description of a healthy mode of life, there are important empirical facts to indicate at least the reasonableness of seeking in individual cases to correct the disease by special means. There is evi-

dence, for instance, of the curative effects in their respective degrees, of cod-liver oil, of climate, of exchanging an in-door for an out-door life, even of the hypophosphites of lime and soda. Ignorant as we are both of the manner and the measure of benefit by these agencies, it is but just that each sufferer should have the opportunity of testing in his own person whether any of them reaches so far as to him. No promise can be held out beforehand of any particular result and, in the case of climate change, the selection of those that shall go and those that shall stay, fallible and often disastrous as it is, is governed for the most part by obvious considerations of common sense.

And if this be true so far, there is surely something to be added upon the side of ethics. Whence do our "decrees of exile" derive their sanction? Does the language of the physician altogether coincide with the character of his knowledge? Is the adoption of a phraseology which recalls the terms and suggests the accuracy of science suitable to the circumstances of the case? Every reader will answer such questions for himself.

We set out with the intention of reviewing the subject of treatment as addressed to the formed disease—phthisis. We have left out of sight all considerations of means of prevention and of the promise which a deeper insight into the pathology of the affection holds out for the future. It is the misfortune of medicine that it is both a science and a calling. At every stage of its progress, however ambiguous and immature the existing knowledge, it must always have a practical side, it must act as well as observe, and be ready to guide where it ought to explore. It hardly needs pointing out, as regards the disease whose treatment we have been considering, that this ever ready translation of principles into practice is a dangerous and fallible process. The facts may be so combined or selected as to favour help or that hypothesis. The hypothesis may be so presented as to favour this or that drug. Regarding medical science as the vulgar regard it, as employed in constructing a code of regulations for the sick needing constant readjustment, it may well appear that our practical gains are small and precarious, representing no fixed principle and dependent only on the temper of the hour. Treatment is at best empirical, and where empiricism fails there is legitimate room and scope for a host of theories and their corresponding remedies. No inference is secure and no practice destined to be permanent. So considered the present aspect of medicine is not bright nor its immediate future assuring. But there is another side where light is already shining and towards which

recent progress in the pathology of phthisis bids us look with almost limitless hope. It is not in the direction of cure but of prevention.

II.—The Hygiene of the United States Army.¹

AFTER the appearance of the earlier statistical reports on the health of the British Army, in 1838-9, the American authorities instituted similar inquiries into the health of their troops from 1819 to 1838, and the results, embodied in an octavo volume, appeared in 1840. A further contribution to these health statistics, embracing the period from 1839 to 1854 inclusive, was published in the quarto form in 1856. Though voluminous documents in connection with the medical and surgical transactions during the Civil War, and reports on yellow fever, and cholera, have since appeared, we have not met with a continuation of these statistical reports before Circular No. 4, issued from the Surgeon-General's Office, Washington, in December, 1870, which embraces descriptions of the various military posts then occupied, including the conditions of their barracks and hospitals, but which gives the strength, and numbers reported sick for the chief diseases at each, for 1868 and 1869 only. These details were preceded by an abstract of the chief hygienic defects at the different stations, by John S. Billings, Assistant Surgeon, United States Army. The volume which forms the subject of the following remarks, issued in 1875, is of the same general character as that just noticed. It opens with a letter from Assistant Surgeon Billings to the Surgeon-General of the Army, giving the state of health of the troops, and also details as to the present barrack accommodation, the food and clothing of the army, and the hospital and medical supplies, and offers suggestions for improvement in these wherever they are deemed unsatisfactory. The views of the various commissions on the condition of the barracks of the British Army, and of the various writers on hygiene in this country and the Continent, are freely used, and it is satisfactory to find that the influence of the advance of hygienic information is so extended, and that the importance of applying this to practice is becoming more generally acknowledged. Mr. Billings' letter extends to fifty-five pages, and the remainder of the volume is occupied with descriptions of the various posts, now amounting to 238,

¹ *A Report on the Hygiene of the United States Army, with descriptions of the Military Posts.* Circular No. 8, Surgeon-General's Office, Washington, May 1st, 1875. 4to, pp. 567.

and extending through the continent from Florida, on the verge of the tropic, to Sitka, on that of the Arctic Circle, and at altitudes varying from the sea level to considerably above 7000 feet. There are many sketches of the posts on a sufficient scale to bring out the relations of these to the surrounding features of the country, and the details of the buildings are rendered clearer by plans, on a larger scale, interspersed through the letterpress, than could be done by that alone. Returns of the chief diseases for the four years, 1870-71 to 1873-74, of the temperature, and rainfall for the same, are annexed.

It has long been understood among army medical officers that their most important function was to prevent disease; but those acquainted with the progress of military hygiene during the last fifty years are aware that the representations of medical officers on this subject received but little attention, even in this country, until after the events of the Crimean war had impressed on the Government and the public the necessity for increased attention to the question. No doubt, after the publication of the first of the series of statistical reports, on the health of the troops in the West Indies, in 1838, important ameliorations were made in the provisioning of the troops, and in shortening their period of service at unhealthy stations, but the stimulus given to hygienic improvement in the condition of the soldier, since 1856, has brought about results favorable beyond what the most sanguine then ventured even to hope for. The difficulties in the way of carrying out hygienic measures were that they generally involved increased expenditure, which those in authority are very chary of incurring, and justly so, unless for an adequate advantage; further, persons, whether military or civil, whose authority it was necessary to obtain before any, even the most ordinary, hygienic arrangement could be enforced, were at best but imperfectly acquainted with its importance, and consequently hesitated to give the weight of their influence in its favour; and, as stated by Mr. Billings, officers were occasionally met with "who were disposed to meet any suggestion from the surgeon as an attempt at interference, and to look upon any attempt to investigate the sanitary condition of the posts as a species of espionage which should be discouraged as much as possible" (p. v). To meet this last difficulty, in the American service, and to give weight to the recommendations submitted by medical officers, instructions were issued by the Adjutant-General at Washington, in November, 1874, directing the medical officer in charge of a post or command to examine into its sanitary condition, together with everything bearing on the health of the soldier, at least once a month, and to make a report in writing to the

commanding officer thereon, with any recommendations he might deem proper. If the recommendations were approved and carried out, the medical officer was to note the fact in a book kept at the post for the purpose; if deemed impracticable or undesirable, the commanding officer was to endorse his objections on the report and transmit it to his superior, giving at the same time a copy of his endorsement to be recorded in the books at the post. Mr. Billings expresses the hope that under this order medical officers may have every facility for acquiring such information as may be required for sanitary purposes, but warns them that, as others have to find the money to carry out their recommendations, they should not be surprised or discouraged at the length of time required to obtain the action desired. This caution is not unnecessary; the Medical Regulations for the British Army, issued in 1859, contain similar provisions to those promulgated at Washington in 1874; but nevertheless, from time to time, considerable friction was experienced in getting even ordinary matters carried out which involved no expense, and which could have been effected by the troops themselves.

As to the health of the United States Army, Mr. Billings gives the strength, and deaths from disease, and from wounds, accidents and injuries for the yearly periods ending 30th June, from 1871 to 1874, and compares the results with those for the civil male population between the ages of thirty and forty, in the thirteen states in which the troops are chiefly stationed, and with the same for the whole United States. The ratios per 1000 living of these stand as below :

	Deaths from disease.	Deaths from wounds, accidents and injuries.	Total deaths.
White troops	10·64	5·13	15·77
Coloured troops	14·13	4·94	19·07
Thirteen States and Territories	5·63	1·59	7·22
United States	6·36	1·04	7·34

Setting aside injuries, to which soldiers are specially liable, the deaths from disease as here shown considerably exceed those among the population at similar ages in civil life under the same conditions of climate, from which he infers that the hygienic conditions under which the troops are placed cannot be considered as satisfactory, and adds :

" If the description of posts following this report be examined, in connection with the statistics furnished with each, the excessive loss by death and discharge, which preventable excess is probably not less than 100 by death and 200 by discharge annually, will not seem unaccountable" (p. viii).

We fear this estimate is rather too favorable; but, un-

doubtedly, there is room for improvement, and it is perhaps advantageous to keep before the public and the authorities a higher ideal than they are likely to reach in practice.

The first bodies of troops or settlers thrown forward into a wild country, occupied by a hostile population, have to depend on themselves to provide cover and protection, and, when large timber is scarce, or at such a distance that their want of transport prevents them availing themselves of it to any extent, they have to resort to expedients which would astonish those not familiar with them, and, as both time and material are scanty, they have to rest satisfied with an amount of accommodation so limited that people in more civilised parts of the world deem it utterly inadequate for the preservation of health; yet, under the double advantage of a favorable climate and the free exposure this mode of life involves, it is often surprising how little sickness is met with; but when things have assumed a more peaceable aspect, and there is no longer the same degree of mental excitement nor call for physical exertion, the evil consequences of restricted accommodation begin to be appreciable. Many of the posts occupied by the United States troops were established, in the first instance, under circumstances such as those described, but have passed into that condition that the restricted accommodation originally provided is now felt to be irksome; others, again, were constructed of more durable materials, but on old plans, which afforded less space and fewer of the accessory conveniences now deemed necessary in a well-appointed barrack, and necessarily involve extensive alterations and much expense to bring them up to the present requirements. All these deficiencies take time to supply, and though there seems to be a desire to do so as fast as possible, there are some notices which cast a doubt as to whether this has been attended to in certain cases as it might have been. For instance, Surgeon E. Swift, stationed at Newport Barracks, Kentucky, opposite Cincinnati, in a special report, states, among other matters:

“Company E, disposable recruits, occupy the third story of another building, 83 feet long, 25 feet wide, and 11 high=22,825 cubic feet. This room has bunks for the accommodation of 120 men, giving 190 cubic feet of air space per man, and is heated in the same manner as the dormitories of companies A and C. It now contains 100 men, who are seemingly comfortable. It is only when more than 200 men are packed into this room, as sometimes necessity demands, covering the floor like red herrings in a box, that discontent becomes irrepressible and desertion results.”

* * * * *

“Company F, unexamined recruits, and the rejected, with those

casually at the post, occupy, at the present time and whenever Company E exceeds 120 men, the post chapel, a small frame building, 44 feet long, 27 wide, $13\frac{1}{2}$ high, and furnished with bunks for forty-nine men."

* * * * *

"When the strength of the command exceeds the capacity of the garrison, it is not alone that the soldier suffers discomfort on account of the crowded condition of the dormitories, but the absolutely necessary accommodations of the hospital, mess-room, prisons, &c., add greatly to his sufferings morally and physically. He is required to wait patiently his turn to each and every place his poor, frail humanity drives him." (p. xvi).

Surely in an old-established post like this, in the immediate vicinity of the city of Cincinnati, means might have been found to obviate such scenes as are here described.

Let us turn from this not pleasing description, and examine into what is proposed to be provided for in the buildings of the future. A board of officers prepared plans for barracks and quarters, which were published in a circular from the Quartermaster-General's Office in September, 1873; according to these a barrack for fifty-eight men, with the usual non-commissioned officers, was to be on two floors, the lower of which was to contain a mess-room, day-room, two non-commissioned officers' rooms, armoury, library, lavatory, kitchen, and cook's room; and the upper, 133 feet long by 23 wide, was to be occupied as dormitory with fifty-eight beds, and would afford 500 cubic feet to each. There is no provision for ventilation except the windows, and no bath-room. While admitting the above to be a great improvement on existing barracks, Mr. Billings considers it a mistake to have limited the space in the dormitory to 500 cubic feet per bed, and to have omitted all provision for personal ablution, and he urges that bathing tubs, or other suitable means for that purpose, should be as regular a part of the supply of a post as bedsteads. In warm countries and seasons ventilation, and bathing (when the water supply is available), involve no great difficulty; but when the weather becomes cold, more especially when the climate is so rigorous as in the greater part of North America during many months of the year, soldiers cannot be induced to adopt either to any extent, unless the air in the one case, and the water in the other, be warmed sufficiently. Both therefore involve a certain expenditure of fuel, and it is useless to look for their practical application unless this be provided, a point which has not yet been fully recognised by the authorities either of the American or British Armies, or, indeed, by most of those who, in comfortable circumstances themselves, recommend ventilation and ablution without

stint for their poorer neighbours. For personal ablution Mr. Billings recommends an arrangement which seems to have been in operation for some time in the prison at Rouen, in France, and to have been highly approved of there; this consists of a central stove surrounded by seven small compartments, into which warm air from round the stove can pass through louvred openings, while the stove serves at the same time to warm the water in a tank overhead; a pipe from this tank is carried into each compartment at its upper part, and being fitted with a rose head similar to that of a common watering pan, the bather can, by opening a cock, cause the water to flow over the body as in an ordinary shower bath. The plan is ingenious, and as the quantity of water required is moderate it deserves a full trial.

Mr. Billings adopts the common view that the excess of phthisis among soldiers over the civil population of the same ages is due, in the main, to the overcrowding and deficient ventilation in barracks, guard-houses, &c. We do not contest the point that these conditions are favorable for the generation of affections of the lungs, but when we consider that the great bulk of the population in this country, if not also in America, have a less amount of space in their houses and dormitories than troops in barracks, and are otherwise under much less favorable hygienic conditions, we cannot avoid the conclusion that too much has been attributed to this cause, and that there is something special connected with the soldier to which this greater liability to phthisis must be due. The nature of his dress and accoutrements constitutes the chief difference between him and persons in ordinary life, and only requires to be mentioned to show its importance. Civilians, when undergoing exertion, generally remove a portion of the dress, and replace it when that is finished, thus preventing a chill when the skin is relaxed and the powers are more or less exhausted; soldiers, on the contrary, go through their usual work, not only with their complete dress on, but often with belts, knapsack, and other impervious materials over it, and, when dismissed from parade, forthwith remove belts, tunic, &c., and run about, too frequently with nothing but a shirt over the back and chest, and that often damp with perspiration, thus reversing the practice of civil life and exposing themselves to a constant series of chills after fatigue, when naturally most liable to be injuriously affected by them, which cannot fail to engender serious pulmonary disease in those predisposed to it, and to impede the complete recovery of those who have already suffered.

The numerous posts at high elevations in the interior of the

United States have permitted their influence on pulmonary affections to be observed, and it may be interesting to many of our readers to learn that—

“The opinion of the majority of medical officers is that altitude and a rarefied atmosphere are not *per se* beneficial in cases of phthisis, but it is the dryness of the air, the exercise, and the out-door life which produce good results in those cases which are able to stand it” (p. vii).

We believe similar views have been entertained as to the beneficial action of the elevated districts in the interior of the Cape Colony on phthical persons.

Mr. Billings enters largely into the question of the food of the soldier, and recommends considerable changes. To appreciate the scope of the regulations on this subject it is necessary to understand the custom of the particular service, and to consider the circumstances under which military operations take place. In the British army it was formerly the custom to issue, in the field, one and half pounds of bread or its equivalent in flour or biscuit, and one pound of meat, leaving the soldier to procure for himself, with his balance of pay, such further articles as his fancy suggested, or the country afforded; while in cantonments, with a smaller ration of bread and meat, a stoppage was made from his pay to provide potatoes, vegetables, groceries, &c., so as to bring up the rations to his requirements. When operations are carried on in countries where additional supplies cannot be obtained it is clear that, to keep troops in working condition, the ration must be sufficient in quantity and variety to maintain the men's health and strength under the exertion they have to encounter, and this must of necessity be issued by the Government, as they have no other means of procuring it. For the same reason, at sea, the full ration must be issued from the ship's supplies.

In the American service the Government ration consists of more than the soldier is supposed to consume; flour seems to be issued in the same quantity as bread is supplied, and as two thirds only of the flour is required for the ration, the saving is employed in the first place to cover the expenses of baking, and the remainder is carried to the credit of what is called the post and regimental funds, in definite proportions, which are expended in educating illiterate soldiers and the children of soldiers, in providing library and newspapers, garden seeds and implements, and in maintaining a band. There is besides what is called a company's fund, being the value of the savings of the rations of the company (exclusive of flour), which is laid out partly in procuring other articles of food to vary the diet,

and occasionally to provide mess and other utensils not issued by Government. The inexpediency of applying the savings on provisions for the latter purpose is pointed out, and it is stated that the desire to show large company's savings tends to restrict the consumption of the rations unduly, and at times excites discontent among the men.

There is a series of extracts specifying the articles comprising the ration of the American troops from 1776 onwards, which are too long for us to do more than merely notice here. That at present in use admits of four variations in the combination of articles, and the amount of nourishment each affords, expressed as water-free nitrogenous and carbonaceous food and salts, is as below :

	Nitrogenous food. Ounces.	Carbonaceous food. Ounces.	Salts. Ounces.	Total. Ounces.
First mode, as usually issued	3·93	16·52	1·19	21·64
Second mode, with rice instead of beans	3·47	16·61	1·14	21·22
Third mode, with biscuit instead of bread	4·99	19·35	1·23	25·57
Fourth mode, with biscuit and bacon	4·10	24·32	1·29	29·71

Surgeon Glover Perin, Medical Director, Department of the Missouri, from whose report Mr. Billings takes the above details, thinks the fourth combination, which embraces twelve ounces of bacon and sixteen ounces of biscuit, should be resorted to only when fresh meat cannot be obtained; excluding this, and taking the mean of the other three, and adding ·256 ounce of extract of coffee, he reckons the average nutritive value of the ration at 23 ounces, which he considers does not contain the nourishment required by soldiers during their average state, varying between rest and activity; and taking the mean of what is required during activity, it is far below it. This deficiency in practice is said to be made up—first, from savings on articles (other than provisions) not expended, which are laid out on the purchase of others more wanted; second, by adding the produce of gardens; third, by the products of hunting and fishing; fourth, by contributions from the men.

Mr. Perin agrees with Surgeon T. A. McParlin, also of the United States army, that a fuller and more varied ration is required, and submits a scheme embracing more bread, with portions of potatoes and fresh vegetables whenever practicable, and if these cannot be had, then fresh or dried fruits to an equivalent value, with fresh or preserved milk, should be substituted. This scheme presents three combinations, and representing their nutritive value in the same manner as above, they stand—

	Nitrogenous food. Ounces.	Carbonaceous food. Ounces.	Salts. Ounces.	Total. Ounces.
First mode, soft bread, fresh meat	4.47	22.15	1.36	27.98
Second mode, biscuit, fresh meat	5.74	26.43	1.44	33.61
Third mode, biscuit and bacon, no fresh meat	4.85	31.40	1.50	37.75

Excluding the third combination for the reasons already indicated in connection with the existing ration, and adding the coffee as in the previous case, the mean ration would amount to 31.05 ounces, which Mr. Perin regards as being 1.865 of water-free food in excess of the requirements of the men, an excess purposely made that the necessary variety may be provided for them. The scale recommended by Mr. Perin seems large, and it is doubtful whether men could continue to consume it unless under continuous hard work. In the Abyssinian expedition the water-free food was rather under twenty-seven ounces daily; and in the Red River expedition, in 1870, it was under twenty-six ounces, but in that instance there were occasional additions of fish, caught in the lakes and streams over which they passed; the exertion undergone by the troops on both occasions, however, was great and continuous, yet they preserved their health and condition under trying circumstances.

The quality of the provisions issued to the American troops is usually very good; the want of fresh vegetables, however, is severely felt at many stations, and as a consequence scurvy is met with more frequently than is desirable. Gardens are cultivated at many posts, but at others where, from soil or climate, the supplies from these are limited or uncertain, the officers are urgent that vegetables, especially potatoes and onions, be added to the ration. When vegetables are scarce Mr. Billings recommends the employment of lime juice, or preserved tomatoes, which he says he found far more efficient than potatoes as an antiscorbutic during the late civil war.

Cooking seems to be done by the men in rotation in the American service, as it was long the practice in our own. The appointment of trained cooks is strongly advocated, and with proper management would, no doubt, conduce to more careful preparation of the food; but inasmuch as every soldier may be thrown on his own resources at any moment, it is essential he should be sufficiently familiar with the essentials of field cooking to be able to shift for himself; this, like many other parts of his training, must be given while at leisure in cantonments.

The clothing of the troops has received much attention, and considerable improvements have been introduced both in the quality of the material and the manufacture of the articles. There seemed to have been parties in America, as in this country,

who, with a very imperfect acquaintance with the requirements of military clothing, recommended a loose fitting dress gathered in at the waist as the most suitable for soldiers. Some of our innovators who pressed such views on the Minister for War in Parliament a few years ago, on visiting Aldershot and inquiring of the soldiers themselves, found them opposed to that style of dress, and a similar objection was raised by the American soldier, as appears by the following passage:—"The plaited blouse is not at all liked by the men; the plaits collect dust and they are hard to keep clean. The new blouse without plaits is generally approved" (p. xlix). The new blouse here alluded to, as shown by a figure in the margin, exactly resembles the patrol jacket now in use by the infantry in this country.

The hospitals in the American army at present are all post hospitals, and, as a rule, are temporary structures, intended to contain twelve or twenty-four beds. Previous to 1872 no specific sum was allotted for hospital construction and repair, but these were included with other buildings and services in the same appropriation, which permitted of the funds intended for them being otherwise applied. Since then, however, 100,000 dollars a year has been allowed expressly for hospital construction and repairs, which is described as a much more satisfactory arrangement than the previous one, though, for the present, the amount is considered insufficient. There are about 140 post hospitals to be kept up; their average cost is about 7000 dollars, and they are expected to last about ten years, during which each may probably cost 3000 dollars for repairs. Under the present system the responsibility for the proper construction and maintenance of the hospital rests primarily with the medical officer, and though his application may not be approved of one year, he is required to repeat it the following one should he consider the alterations or repairs necessary for the proper treatment of the sick.

The plan and details of the post hospitals were laid down in a circular issued from the Surgeon-General's Office 29th July, 1871. According to this each hospital is to embrace a central portion of two stories, containing the administrative offices and accommodation for steward and attendants, and one or two wings (each being a ward for twelve beds), as the hospital is to be for twelve or twenty-four patients. The kitchen and mess-room are on the first floor of a projection from the rear of the centre building, and over these on the second floor is a room for violent patients, a very necessary appendage to a military hospital, where there are no detached wards that may be used for that purpose. The buildings are what are called frame houses,

with shingled roofs, and the lower floor is raised about three feet from the ground, and surrounded by a wide verandah. The wards are forty-feet long by twenty-four broad, and fifteen high in the clear from floor to ceiling (apparently ten feet from floor to wall plate); this gives 108 square feet of floor, and about 1350 cubic feet to each bed. Ventilation is secured in summer by free openings at the ridge; during cold weather these are closed, and air admitted from without is passed round a stove to warm it, while the upper part of the stove pipe passes through a metal cylinder in the roof, communicating with the interior, which serves as outlet for the vitiated air from the ward. These modes of warming and ventilation were extensively used in the hut hospitals employed during the civil war; they seem to give satisfaction while the external temperature is not very low, but when that approaches zero, it is remarked, that it is impossible to obtain both satisfactory heating and ventilation by any stove or fireplace in the ward itself, and that therefore it becomes necessary to sacrifice ventilation to some extent.

A modification of the Galton grate, consisting of two such placed back to back, with the air-heating chambers between, was tried; some of these set up in twelve bed wards, at Forts Whiffle, and McHenry, caused a change of air in the rooms of about 36000 cubic feet an hour, and maintained an adequate temperature except when that of the external atmosphere fell to zero, but it was found they did not afford sufficient heat when the external air was below the freezing-point, unless with anthracite as fuel.

A new hospital, called the Barnes Hospital, is being erected at the Soldiers' Home near Washington; being intended for a permanent establishment, it is built of brick, and will accommodate fifty beds, and an attempt has been made, in its arrangement, to solve some of the problems of hospital construction now discussed in civil life. The building consists of a central block, containing the administrative departments and accommodation for attendants, mess-room, &c., and two wings containing wards, with the accessory offices at their detached ends, on the general plan of the post hospitals already alluded to, though with the accessory portions considerably more developed than in the post hospitals. The elevation shows a basement, over which there is, in the centre block, three stories, with attics in addition, and in the wings two stories, with attics; the purpose for which the attics are intended is not stated. The basement floor seems entirely occupied with arrangements for the admission and warming of air; the wards on the first floor have no divisions, those on the second floor are each subdivided into four larger and four smaller apartments, extending along the walls,

with a passage in the centre between them, and apparently intended, the larger for two beds and the smaller for one only. The warming is to be effected by a hot-water apparatus, consisting of two boilers and thirty-five coils of cast-iron pipes, with the ordinary communications from and to the boilers, the maximum temperature at which is not to exceed 210° Fahr.; the coils are placed round the basement, in connection with uptake flues to convey the heated air to the apartments above, and are intended to maintain a suitable temperature during the coldest weather in all the rooms, corridors, &c., in the building, the kitchen and laundry excepted. The coils under the wards are to be capable of heating 60,000 cubic feet of air for each, per hour, from zero to 85° (*sic*) Fahr.

"The fresh-air flues for the lower wards are double, one opening near the floor, the other near the ceiling, and can be controlled by registers, so that the air can be introduced at top or bottom. This arrangement is intended for experimental purposes, and will be modified in accordance with the results obtained by trial."

The removal of the vitiated air from the wards is

"... effected by two chimneys, six feet square and one hundred feet high, in the centre of which, from base to top, runs a smoke flue of boiler-iron two feet in diameter. Into these iron tubes open the boiler smoke flues, as above provided for: the smoke flue from the engine, and, in the third story, the flues from the kitchen and laundry furnaces. These smoke flues have also extra grates at the base."

"Into the aspirating space in these chimneys open the foul-air flues, which are tubes four feet wide by twelve inches deep, running from end to end beneath the floor in the centre of each ward. These flues or boxes have registers opening into the ward above through the floor, and into the ward below through the ceiling. These registers close air-tight, so that the air from any ward can be taken out either above or below."—

"The clothes lift, and main lift, and dumb waiter open above into the main ventilating stacks. The ventilation of the centre building is by a large Emerson ventilator over the central hall and staircase, and by fireplaces in the rooms. The end towers, containing the ward offices, have independent ventilation by aspirating chimneys heated by furnaces in the basement which supply hot water to the lavatories and bath-rooms. The water-closets are ventilated downwards through the seats, except in one set, where the Jennings closet is placed for the purposes of comparison. The gas jets contribute to the ventilation, having flues over them which connect with the foul-air ducts" (pp. lv, lvi).

An ingenious method of obtaining a low temperature in cases of fever has been employed by Assistant Surgeon A. H. Hoff.

A hole, about a foot square, is cut in the floor under a bed, and made to communicate with the external air; the incoming current is broken up by any perforated material, so as to diffuse it around the bed. "In cold weather, and with a thin mattress, Dr. Hoff has in this way obtained a local reduction of temperature of the air of 35° Fahr., and has treated cases of typhoid fever in such with good effect."

The mode of supplying the diet for men in hospital is much the same as that followed in the British army many years ago. The hospital fund is credited with the value of the ordinary rations of the men while sick; the medical officer draws as much of these in kind as may be necessary, the value of which is charged against the fund, and he is allowed to expend the balance in providing additional articles of food, such as milk, eggs, butter, fowls, &c., as the cases may require. In addition, however, arrowroot, farina, corn starch, tea, and sugar, are supplied by Government. "With this and occasional dishes from the table of the medical officer for special cases, it may be said that the sick of the army is satisfactorily fed."

Our readers may perceive from the above remarks that great attention is being directed to the improvement of the hygienic condition of the American army, and, as this is effected, we may expect to find as remarkable an improvement in the health of their troops as has followed the adoption of similar measures among those of this country.

III.—Physiology of the Uterine Mucous Membrane and Placenta.

(Continued from vol. lxviii, p. 143.)

WE come now to consider the changes which take place in the inner surface of the uterus after it has become the nidus of an impregnated ovum. The earliest impregnated uterus hitherto examined has been described by Professor Reichert, of Berlin. A girl, aged 21 years, had been regular until within a fortnight of her death, which took place from poison on November 21st. According to her mother, she expected her catamenia on November 7th. Her lover was called away for three years' military service in the first days of November. The menstrual flow due on November 7th was the first absent one. Reichert believes that ova are extruded and fertilised only about the beginning of a regular menstrual discharge, and accordingly he concludes that impregnation occurred, in the above

case, thirteen or fourteen days before death. In the uterus he found the following appearances :—The decidua vera was somewhat more developed than the decidua menstrualis at the height of its formation in healthy women, yet apparently of similar nature and shape to the mucous surface. The openings of the Fallopian tube were not closed by this membrane. At the os internum, the decidua terminated by an abrupt line in the mucous membrane of the cervix, the latter being unchanged; the inner surface of the body of the uterus was divided into a number of islets, the surfaces of which were flat, but divided by more or less deep furrows into sections, and these again into primary or secondary wart-like or papilla-like elevations. The formation of these papillæ is constant in the mucosa of the impregnated uterus. The primary papillæ vary in size, are of short cylindrical shape, covered at the free end by papillæ standing thick and close together. The latter are of nearly equal size, have likewise a short cylindrical form with a round free end. Reichert states that he has never met with these appearances in the decidua menstrualis, and therefore regards them as characteristic marks of the decidua vera. These observations have been made in one uterus of two weeks' gestation only, and we must await further observations in order to be able to estimate their exact value.

The microscopical appearances presented by the decidua vera during the first month of pregnancy are similar to those seen in the decidua menstrualis, but Dr. Engelmann says "the proliferation of the interglandular tissue extends to a greater depth, and in the upper layers is still more marked than in the menstrual membrane; the individual cells are more enlarged almost throughout the whole membrane, and in many the nuclei are found in the various stages of segmentation indicative of active cell proliferation."

The epithelial lining of the uterine cavity is still ciliated and columnar, but the glandular epithelium has undergone some change.

The ovum, in its passage from the Fallopian tube to the cavity of the uterus, does not carry before it a reduplication of the mucous lining of the uterus, for the mouths of the tube are patent; nor does it, after entering the uterine cavity, become lodged in the orifice of one of the glands, as was supposed by Coste. Having entered the uterus it becomes fixed to some spot on the surface in the neighbourhood of the fundus, probably by being lodged in one of the furrows which are to be seen at that time in that situation. Once fixed, a fold of the decidua forms around it; this gradually increases and spreads itself over the ovum until the folds of opposite sides meet and

unite. When this union has taken place the ovum is completely encapsuled. The decidua, at the place where the ovum is attached, becomes thinner than the neighbouring part, and assumes a saucer shape. There is a shallow depression in it corresponding to the shape of the ovum. This may possibly be due to pressure exercised on a very soft tissue by the ovum. The decidua reflexa has a structure similar to or identical with that of the vera. It is extremely thin at the point of union of the opposite folds, and thickest close to its origin in the decidua vera. Both surfaces are similar to that of the vera, and have glands opening upon them. The mouths of the glands are elongated and sometimes appear as irregular fissures, and are more scanty than in the vera. The glands themselves are distorted, and their course is not perpendicular through the thickness of the reflexa, but more or less parallel to its surface, for they run down into the decidua vera. The glands are more numerous towards the base of this structure than over its surface. The structure of the scrotina is similar to that of the vera, but its inner surface becomes roughish and somewhat irregular at an early period of gestation.

The scrotina contains glands which open on the surface, but apparently in fewer numbers than the vera.

During the three first months of gestation the uterus grows more rapidly than the ovum, and the cavity contains, not only the ovum, but a quantity of fluid also—hydroperione. After the first month the decidua does not maintain its former thickness, but becomes somewhat thinner, though it still preserves its convoluted appearance. The superficial layers remain compact, the necks of the glands become narrower, whilst the deeper layers of the membrane become looser and more spongy, the calibre of the glands increasing, and the septa between them diminishing in thickness. The cells forming the superficial layers of the decidua increase in size. In the glands the epithelium is in places preserved, whilst in others it is wanting. In the second month Dr. Engelmann was unable to find any of the surface epithelium, but could not state if it was normally wanting, or if the condition was due merely to imperfect preservation of the subject. The reflexa also diminishes in thickness.

Dr. Engelmann says :

“The diminished thickness of the reflexa seems to indicate that the growth of the ovum is more rapid than that of the membrane enclosing it. This is, indeed, the case ; but the development of the reflexa has not entirely ceased, as even a macroscopic examination of the specimens will show, and the condition of its cells and the segmentation of their nuclei is a still more conclusive proof of their activity and growth.”

In the fourth month the ovum has attained a size sufficient to fill the cavity of the uterus, and from this time onward the growth of the ovum exceeds that of its containing organ. The walls of the uterus, which have increased in thickness until this period, begin now to grow thinner owing to the distension caused by the growing ovum, until at term they are somewhat thinner than the walls of the unimpregnated organ.

About the fifth month the chorion, reflexa and vera, become more or less united, and later their union becomes so firm that the several membranes cannot be separated without laceration.

The condition of the glandular epithelium from this time forward is somewhat uncertain. Friedlander states that the glands are lined by perfect epithelium, partly cylindrical, partly tessellated, while Engelmann maintains that, even at the fourth month, the glandular epithelium has in great part disappeared, or been changed into granular nuclei, whilst at the ninth month no trace of it can be found, except at the fundi of the glands.

Before parturition the membranes undergo fatty degeneration. This change is most marked in the superficial, the deeper layer being less affected. It is not known at what period of gestation this retrograde change begins; but it has been observed in the maternal membranes in the last month of pregnancy, while in the fetal it has been seen much earlier.

The Placenta.—The human placenta is an organ of such great complexity of structure, and the tissues entering into its formation are of so delicate and fragile a nature, that the difficulty of unravelling its anatomy has been very great. It cannot be wondered at, therefore, that views very different and even opposed have been and still are held with regard to its structure. The two courses of lectures delivered at the Royal College of Surgeons in 1875 and 1876, by Professor Turner, of Edinburgh, on the comparative anatomy of the placenta, are therefore heartily welcome, for they contribute in no small degree towards settling one of the most difficult questions in human anatomy. The first three lectures which form the first series contain a description of the chorion and the mucous membrane of the unimpregnated uterus, and of the three simplest forms of placenta—the dif-fused, the polycotyledonary, and the zonary. In the second series, which were delivered last June, but which are not yet published, Professor Turner described the remaining forms of placenta, including that of the human female. The most simple form of placenta is the diffuse, in which the villi of the chorion are scattered over the whole or nearly the whole surface of the ovum. Of this variety those of the pig,

the mare, orca gladiator or narwhal, and others were described. In the pig the surface of the chorion was for the most part covered by small vascular ridges which, at a later stage of gestation, became villous. These ridges were arranged in a stellate manner around a number of small, circular, raised non-villous patches, into which the glands opened. The ridges often branched and united so as to form a network. The spots around which the ridges were arranged were less vascular than the ridges themselves, and were surrounded by a vascular ring. The maternal placenta had an undulatory appearance owing to numerous shallow fossæ separated by intervening ridglets. Professor Turner says :

“In a more advanced specimen, where the fœtus was six inches long, the mucosa was thrown into transverse folds. When examined with a pocket lens, fine ridges and furrows were seen, which were adapted to furrows and ridges on the surface of the chorion. When more highly magnified the furrows were seen to be subdivided into shallow spots into which the villi of the chorion fitted. Scattered over the surface of the mucosa were numerous smooth, almost circular depressed spots, free from ridges and crypts, corresponding in size and numbers to the circular star-like spots on the surface of the chorion; and, as Von Buer and Eschricht have already described, the starlike elevations of the chorion are adapted to these smooth spots on the mucosa when the two surfaces are in contact. These smooth depressed spots were very definitely circumscribed by the crypts which immediately surrounded them. In the minutely injected uterus the ridges and the walls of the crypts were seen to contain a compact capillary plexus, whilst the smooth spots possessed a feeble vascularity, so that they appeared as distinct white spots, surrounded by highly vascular ridges on the injected mucous membranes. Beneath the superficial crypt-layer of the mucous membrane was a well-defined glandular layer, the glands in which were tubular and branched repeatedly, so that each gland stem or duct had connected with it numerous branching tubes. The depressed circular spots had a special relation to the ducts of these glands, for opening either in the centre of each spot, or near its border, by an obliquely directed orifice, was a gland stem, which could be seen running somewhat tortuously from the deeper glandular layer of the mucosa to the spot.”

In the pig the glands did not open into the crypts, and consequently the crypts were not dilated mouths of glands, but were formed by modifications in the interglandular portions of the mucosa. These crypts were not met with in the unimpregnated uterus of the pig. They were lined by columnar, probably ciliated epithelium, were very shallow, and owing to this and the shortness of the villi, their maternal and fœtal membranes could easily be separated. The villi did not enter the

glands, nor did the glandular secretion come into immediate contact with the villi, but with the non-villous patches found on the chorion.

In the pig the villi were short, simple, and arranged on radiating ridges; in the mare they were filiform, and gathered into brush-like tufts separated by narrow non-villous intervals; in orca gladiator they were club-shaped, sometimes arranged in rows, sometimes in tufts. The shape and arrangement of the crypts on the surface of the mucosa corresponded to that of the villi in the chorion. In the mare they were distributed in areas which were separated by smooth ridges. On these ridges the glands opened. In orca the surface of the mucosa was irregularly divided by slight folds of membrane, and the crypts were distributed in the areas between these folds, but also in the folds themselves, and glands were found opening into crypts. The crypts were, however, far more numerous than the glands, and, consequently, there was a large number of crypts into which glands did not open, the mucosa of orca being more uniformly crypt-like than that of the pig or mare, so that there were no spaces free from crypts on which glands could open. According to the observations of Eschricht, however, who examined the gravid porpoise, the glands open in cetacea, not into the crypts, and the latter must be considered interglandular.

The epithelium lining the crypts in the pig was seen to be columnar; in the mare it was partly columnar, partly of an irregular shape, and partly hemispherical in form; in orca the epithelial cells had a transitional form between the cylindrical and tessellated varieties.

In all the forms of diffuse placenta examined the villi of the chorion did not enter into the glands, but into crypts which were formed on the mucous surface after impregnation; and the secretion of the glands did not come into immediate contact with the villi except in orca, when the glands opened, in some instances at least, into crypts. In all the others the crypts were interglandular.

The glands open on distinct spaces in the polycotyledonary placenta; the chorion villi are gathered into a number of tuft-like masses called foetal cotyledons. These are lodged in an equal number of crypts in the mucous membrane of the uterus—maternal cotyledons.

The cotyledons vary in shape and in number in different animals. In the sheep they appeared as “cup-shaped mounds” on the uterine wall. They consisted of a soft, spongy material, divided into numerous pits, were covered on the outer surface by mucous membrane and lined by cells of various shapes, some being cylindrical.

In the cow the cotyledons were fungiform, and connected to the mucosa uteri by a thick stem. The under surface was riddled with pits. In the deer the cotyledons were convex, as in the cow, but sessile. They were highly vascular, and in the connective tissue beneath the cotyledons of the sheep corkscrew-like arteries have been seen. The vessels in the cotyledon itself passed up to the surface in the connective tissue of the walls of the pits. They frequently branched and ultimately formed a capillary plexus, but did not dilate into sinuses.

The utricular glands did not open into the pits in the cotyledons, but on the intercotyledonary mucous surface. Gland openings were also seen on the outer mucous surface of the cotyledon of the sheep and on the stem of the cotyledon of the cow. Glands were found, but not in great numbers, in the connective tissue beneath the cotyledons, but they appeared to be branches running obliquely, and not opening into the crypts of the cotyledon.

It has been supposed that the cotyledonary crypts of this form of placenta were the dilated mouths of glands, but it is almost universally agreed now that they are depressions formed after impregnation in the mucosa uteri.

The foetal cotyledons consisted of numerous vascular branching villi, which varied in shape and size according to the shape and size of the maternal cotyledons into which they entered. When the villi were withdrawn from the pits they carried with them some of the maternal epithelium, so that the polycotyledonary placenta is deciduate, while the diffuse variety is non-deciduate.

The zonary placenta is much more complex of structure than either the diffuse or polycotyledonary form. It is met with in the carnivora, pinnopedia, and some other animals. Professor Turner gives the following account of the naked-eye appearances of the pregnant uterus of a carnivora :

“The gravid uterus of the dog, cat, and other pluriparous carnivora possesses a moniliform appearance. Each dilatation is a compartment of the uterus containing an embryo with its membranes, and between adjacent compartments the uterine cavity forms a narrow tube. If one of these compartments be opened in a well-advanced stage of development of the embryo, the chorion will be seen to be smooth and bare of villi, except in about its middle third, where the villi are arranged as a zonular band around the transverse circumference of the chorion. The uterine mucosa possesses a similar zone closely blended with the zonular band of the chorion. The mucous membrane on each side of the zone is smooth and vascular ; it lies in apposition with the smooth part of the chorion, but has no attachment to it. Where the zonary and smooth parts of the mucosa are continuous with each other a narrow strip of mucous membrane is reflected on

the margin of the zonular band of the chorion, and forms a rudimentary decidua reflexa."

In these lectures the zonary placenta is described as seen at some period of gestation in a considerable number of animals, but that of the cat is more fully described than any other. Indeed, the steps in the process of the formation of the placenta in this animal are traced from an early period of gestation up to the time of parturition.

At an early period of gestation in the cat the mucous membrane towards the poles of a compartment in which an ovum was lodged was smooth, and the placental area was marked by very delicate reticulations and studded with minute orifices. These were the mouths of the crypts within which the villi of the chorion were lodged. The surface was covered by short columnar epithelium. Glands were found in the connective tissue beneath the crypts, and were lined by columnar epithelium; but their orifices could not be traced. It was, however, ascertained that the crypts in a given area were much more numerous than the glands, and though some glands may open into crypts in the cat as in *orca*, yet the crypts must be considered interglandular. As development proceeds, the smooth patches at the poles of the chorion increase somewhat in size. The crypts become more dilated, deeper, and more subdivided, and the laminae between them larger. The epithelium lining the crypts remained in part columnar, and in part had become "irregularly polygonal." The subepithelial connective tissue was vascular. Till nearly half the period of gestation had been completed the villi of the chorion could be easily separated from the crypts in the decidua; but about the half term this could no longer be done, and very important changes were found to have taken place. The villi were limited to the middle third of the chorion, and they were so interlocked with the corresponding zone of the decidua that the two parts of the placenta could not be disengaged from one another.

The villi passed almost vertically through the placenta up to its uterine surface, and the laminae of the maternal tissue forming the walls of the crypts passed between the villi, forming sheaths for their branches and stems up as far as the chorion. There was a well-defined decidua. The blood-vessels of the scrotina ramified throughout the laminae of the maternal placenta, and when injected they were seen to run up almost vertically to the foetal surface of the placenta, and the capillaries were "dilated to two or three times the size of the capillaries of the foetal villi," and near the foetal surface they dilated into sinus-like enlargements.

In the fox the dilatation of the capillaries was still more marked, and there can be little doubt that these dilated capillaries represented the maternal blood-sinuses which find their maximum development in the human placenta.

Thus it will be seen that the maternal part of the placenta in the cat and carnivora passes through the whole thickness of the organ as far as the chorion, that there is an intimate apposition of the surfaces of the chorion villi and of the maternal crypts, and that by this interlocking of the two parts of the placenta the foetal and maternal bloods are brought into near proximity. The maternal blood-vessels in some zonary placentaë are still covered by a stratum of connective tissue, as well as a layer of epithelial cells; but in the fox some of these vessels were found surrounded by a ring of epithelial cells only, the connective tissue stratum having become indistinguishable or having entirely disappeared. This condition will be seen in a more marked degree in still higher forms of placenta.

These lectures are of the greatest value as a contribution to our knowledge of the structure of the placenta. It is only by such studies in comparative anatomy that it is possible to unravel the structure of the human placenta. Not only is the latter organ at term of so complicated and so fragile a structure as almost to defy the inquiries of the anatomist, but it is fortunately also only very rarely that specimens of the organ *in situ* can be found at term or at any other period of gestation; consequently it has not yet been possible to trace month by month the steps by which the human placenta is formed from the earliest attachment of the villi to the maternal tissue till the organ is fully developed. In consequence of this it is necessary in this instance, as in many others, to call in comparative to explain human anatomy.

In this series of lectures the structure and mode of development of the simplest forms of placenta are described; and we may infer with tolerable probability what goes on during the early period of gestation in the human subject. The course, however, is not yet complete. In his description Professor Turner has only reached that stage when the maternal capillaries have begun to enlarge; they have not yet become great sinuses, although traces of sinus-like dilatations were seen in some specimens of the zonary placenta. The further development of the placenta met with in the bell-shaped and discoidal varieties was described in a second series of lectures delivered at the Royal College of Surgeons in June last.

The series before us is illustrated by remarkably good coloured lithographs of sections of injected placentaë and by some woodcuts.

IV.—Cerebral Physiology and Epilepsy.¹

THAT the cerebral convolutions should have to do with special muscular movements and also with the processes of thought seems on its first announcement so strange a doctrine that the profession can scarcely be said, even yet, to recognise its true significance. It was easy enough to imagine that the convolutions had either function singly, but the alleged combination of spheres of action so different introduced an element of such confusion that few sought to overcome the difficulty involved in the proposition.

Many well-known facts in clinical medicine had indeed long pointed in this direction. For instance, it had become an aphorism, much insisted on by Dr. West, that "convulsions in the infant answer to delirium in the adult." Here then was a striking parallel drawn between muscular and mental perturbations. But perhaps nothing served to call attention so markedly to this subject as the elaborate study which has been made in recent years of cases of aphasia. This affection presents such a frequent combination of paralytic or convulsive muscular disturbance, with alterations in the power of speech, as to excite the most lively interest of physiologists and psychologists alike; this alteration in the power of speech is, moreover, found to vary in such a remarkable manner as to present nearly every gradation, from a purely paralytic affection of the muscles of articulation up to a complete loss of the intelligent recognition of words and of their voluntary use. Not only was aphasia found to be often associated with paralysis of the limbs, but when thus associated it was almost invariably the right side which was so affected; and from the pathological side, the lesion in the brain came to be localised with great certainty not only in the left side, but even in certain parts of the frontal convolutions. Thus we had a localised affection of the convolutions associated with special muscular disturbances in the right arm, and with mental disorder of such a special kind as almost to be limited to the use of words.

¹ 1. *Clinical and Physiological Researches on the Nervous System* (Reprints). No. I.—*On the Localisation of Movements in the Brain*. By J. HUGHLINGS JACKSON, M.D. London, 1875.

2. *On the Scientific and Empirical Investigation of Epilepsies* (being the introductory chapters of a forthcoming work on Epilepsy). By J. HUGHLINGS JACKSON, M.D. 'Medical Press and Circular,' 1874—1876.

3. *Contribution à l'Etude des Convulsions et Paralysies liées aux Méningo-Encéphalites Fronto-pariétales*. Par LOUIS LANDOUZY. Paris, 1876.

4. *Experiments on the Brain of Monkeys*. By DAVID FERRIER, M.D. 'Proceedings of the Royal Society of London,' vol. xxiii. London, 1875.

Meanwhile Dr. Hughlings Jackson, having made an elaborate study of the disorders of speech observed in hemiplegia, chorea, &c., had likewise been pursuing the subject of localised spasms and convulsions, as affording specimens of the simplest form in which the study of epilepsy could be approached. After various speculations and hypotheses he was led, by clinical and pathological study, to regard the cerebral convolutions as containing the centres for the regulation of certain definite and methodical movements in the limbs and face; the regulation, that is to say, not of the contractions of certain muscles merely, but rather of special groups of muscles co-ordinated in a special manner. It is clear that, if this be so, such centres must be closely related to other similar centres, some with a more comprehensive, others with a more limited range of operation; for even such a simple action as bringing the hand to the mouth calls into combined operation adjusted muscular movements of the arm and hand, with associated actions in the trunk and face, any of which might be called into play separately, or in quite different combinations. Not only must there, in such a view, be manifold and intricate connections between the different centres thus localised (by the hypothesis) in the convolutions, but all these again must be represented in the great motor ganglia of the brain—the corpora striata—through which the impulse to movement is transmitted to the nerves and muscles. In studying convulsive or paralytic affections from this point of view, we must consider whether the mischief exists in some of those higher centres in the convolutions which have manifold relations with other motor centres, and with the sensory centres which bring the individual into relation with the outer world; or whether the mischief does not remain confined to one of the lower centres whose operation is comparatively limited; or finally, whether the mischief affects the great communicating tracts in the brain which transmit motor and sensory impulses to and from all parts.

Dr. Hughlings Jackson insists on a distinction which must be clearly understood in the study of his views, as it finds a constantly recurring place in all his writings. Nervous lesions are classified by him according as they destroy the function of a part or merely pervert it. With regard to the “destroying lesion” there is not much difficulty; it is easy to understand that, if a nervous centre or a communicating nervous tract be destroyed, in whole or in part, its function is diminished or destroyed in a corresponding degree. But the obvious manifestations of such a destroying lesion will vary according to the position at which the lesion has occurred. Thus if it be the musculo-spiral nerve which is destroyed, only the muscles ani-

mated by it will be paralysed ; if the motor tract within the brain in relation with this same nerve be destroyed, all movements depending on this nerve, and on other nerves similarly related, will be paralysed, more or less deeply according to the severity of the lesion ; if one of the centres in the convolutions which regulate certain movements of the fingers be destroyed, then those special movements will be affected, the lesion dissecting out, as it were, certain branches of nerves and certain fasciculi of muscles : if one of the highest centres in the convolutions be the site of the lesion, there may be no obvious paralysis of any one movement, but only a degree of awkwardness of it in certain combinations or under certain circumstances : this may leave no very obvious paralysis, but the lesion may be fatal to the use of the part for some of its higher purposes. So far the effect of destroying lesions can be readily conceived ; the principle of compensation, of supplement from the other side of the brain, and from the overlapping of one centre by another being the main connection to be borne in mind.

But the function of a nervous centre may be perverted without being destroyed. The cells in the grey matter of the brain are supposed, while in healthy action, to store up nervous force, each centre giving it out when required in a definitely ordered manner, and under control of those centres which operate on it from above, and in due relation to the sensory or reflex impulses from all quarters which may be transmitted to it. Disordered nutrition, however, may lead such nerve centres to discharge their impulses on unduly slight provocation, or, it may be, in some abnormal manner ; or, again, this discharge of nervous influence from an abnormal centre may perturb the action of otherwise healthy centres, and so lead to their discharge. The conditions leading to such perturbations are spoken of by Dr. Hughlings Jackson as “ discharging lesions,” and to them he attributes the “ clotted mass of movements ” which we term a convulsion, as well as localised twitchings which may only affect special muscles ; the ill-regulated motions observed in chorea, and the continuous contractions which constitute a spasm, can be referred to similar causes. The difficulties which beset the study of these “ discharging ” lesions, at least as throwing light on the localisation of movements in the brain, are very great. Convulsions limited to a limb, or to a part of a limb, are, of course, comparatively rare ; even in the more common case of the convulsion invading one part very markedly before the rest, the physician is seldom at hand to note the phenomena critically, and amidst the general excitement attendant on such seizures, few have the power of noticing accurately these details,

and consequently the clinical part of the study is much hampered. On the other hand, the pathological investigation encounters this immense difficulty, that the lesions must almost necessarily be very slight, inasmuch as they are compatible with the exercise of the function of the part, although, indeed, in a perverted fashion. Even when a tumour, for example, is discovered in the brain, it can only be in an indirect way the cause of convulsions, for it existed before the attack, and it may remain without the recurrence of such symptoms for a lengthened time. In this case the tumour is not itself the "discharging lesion," but from pressure, softening, vascular interference, or such like, it is the occasion of the occurrence of instability in the adjacent grey matter which, on some slight provocation, explodes, as it were, in a series of convulsions. This combination of clinical and pathological difficulties no doubt accounts for the comparatively limited amount of attention awarded to the painstaking researches of Dr. Hughlings Jackson, and for the slowness with which for many years his doctrine of chorea, convulsions, and epilepsy made its way.

When, however, it became known that Ferrier's experiments on the brains of animals consisted in the artificial stimulation of the cerebral convolutions, and that this stimulation was followed by definite movements, usually of a purposive character, an immense assistance was at once given by the physiologist to the speculations of the clinical observer. For it was evident that the application of slight currents from an induction apparatus, carefully localised to special convolutions, was a very close imitation of what had been designated a "discharging lesion"; indeed, Ferrier's experiments were confessedly undertaken to imitate in healthy animals what Hughlings Jackson had described as occurring in the epilepsy of man. Some doubts have, indeed, been expressed as to the conclusions to be drawn from these experiments; but, in any case, the facts remain clear,—localised stimulation of special parts of the convolutions gives rise to very special and localised combinations of muscular movements. To those of us who have seen the very great degree of precision with which movements of the eyelid, of the tongue, and of the hand can be predicted before the application of the electrodes to a given spot on the surface of the brain, it is perfectly clear that such portions of the cerebral cortex must stand in some very intimate connection with the government of these movements. The brilliant results, therefore, obtained by Ferrier in mapping out special parts and convolutions of the brain as governing special movements of the limbs and face, have verified not merely the main doc-

trine announced by Hughlings Jackson, but have even corroborated many of his conclusions in detail, and they naturally add great weight to the conception of epilepsy at which such an acute observer had arrived.

Why then, it may be asked, did these views, now admitted by all to be so weighty, fail to arouse general attention or assent? It must be confessed that for this Dr. Hughlings Jackson is himself somewhat to blame. His writings on the subject are so numerous and are scattered through nearly every form of the voluminous periodical literature of the kingdom, that few persons could find the time or the opportunity to follow the writer from journal to journal. It is, therefore, with much satisfaction that we find that he has determined to issue reprints of several of his most important papers, the first number of which stands at the head of this article. The other series of papers in the 'Medical Press and Circular,' constituting the first seven chapters of a work on Epilepsy, now apparently in progress, gathers up in a compact and yet complete form the author's most recent views on cerebral physiology and pathology. We confess, however, that there is something more than the difficulty of ferreting out the author's papers from the different volumes of journals and transactions. Even the two works just named, with all the advantages of reproduction, do not present the author's views in a convenient form. The article 'On the Localisation of Movements in the Brain' is overladen with a preface nearly double the length of the reprinted article, and this preface is itself in large part a reprint. Both preface and article are, moreover, so much burdened with footnotes and with quotations from previous articles of the author, and from metaphysical and other writers, that it needs a clear head to keep one's way in reading this little pamphlet. Similar remarks apply to the very able series of chapters on Epilepsy, although in this more systematic work the hardship is less felt. The author seems unable to think of or at least to write on his subject on the basis of what he now believes; he is constantly quoting opinions announced long ago, containing the essence of his doctrine, mingled, however, with little and sometimes important differences which are most distracting. These references are not introduced in order to claim priority or credit for his previous speculations, but evidently because, when verified by subsequent experiment or observation, they seem to be thus stamped with special value. And no doubt such is really the case. But, however important to himself the history and development of his opinions or speculations may be, it introduces an undue complication and calls upon the reader to master not only the views which he

now regards as true or probable, but those also which he now regards as imperfect or faulty, however interesting.

In addition to all these difficulties of style there remains, of course, the difficulty of the subject itself, and this is, no doubt, the real cause of the complicated efforts made to explain it. For, to fall back on our opening proposition, Dr. Hughlings Jackson believes that the cerebral convolutions are the centres concerned in muscular movements as well as in the processes of thought, and further, that it is *because* the convolutions contain centres for the processes which represent movements and impressions, that they are fitted to be the "organ of mind." He shows very distinctly, and he quotes metaphysical writers in his support, that even in vision we have not to do with purely sensory phenomena, but have mixed up with them a motor element, connected with the muscular adjustment of the eyes in looking at the object; the result conveyed to the mind is a compound of the incoming nervous impression proceeding from the retina, and of the consciousness of an outgoing energy proceeding to the ocular muscles. It is needless to enter into details as to how touch and sight co-operate in giving correct ideas; enough has been said to justify the use of the phrase "sensori-motor processes" as applying both to those processes which appear to be chiefly motor, and to those which seem chiefly sensory.

It may be conceded by some that stimulation of "sensori-motor" centres occurs when objects are actually seen, words actually pronounced and heard, or things actually felt; but it may be asked, what evidence is there of such stimulation when things are only remembered or thought of? A striking indication of the effect of this kind of stimulation of the nerve centres is obtained by faradising stumps: "A man loses his arm by amputation just below the elbow; he knows nothing of anatomy; yet when the end of his ulnar nerve is faradised (the stump being healed) he describes the *movements which we should see* if we faradised the ulnar nerve in a healthy man." Here we have stimulation of the centres, through the nerve, giving rise to the *idea* of movements in a vivid and accurate manner, apart from any such movements actually taking place, or, indeed, being possible. In this case we can trace the stimulation which gives rise to the imagination; but other occult stimuli might, no doubt, excite these centres in a similar way. Something allied to this kind of excitation is seen when those but little accustomed to read, or when children learning for the first time to do so, are observed to speak aloud as they read to themselves; this may gradually merge into an inarticulate muttering sound, and may finally linger as a slight

movement of the lips; even when this has ceased, there may be a more or less continuous consciousness of an incipient framing of the words with the tongue. The centres connected with the audito-articulatory processes are thus stimulated into more or less distinct action during the process of reading,—the visual excitation being translated into an audito-articulatory excitation, just as one but little versed in a foreign tongue finds that while reading or speaking he is translating as he goes along from the one language into the other. Additional confirmation may also be obtained from the case of deaf mutes, who, it is alleged, may be seen at times to make faint movements with their fingers equivalent to the inarticulate motions of the lips just referred to. All this points to a connection of a most vital character between the processes giving rise to muscular movements of the tongue and lips, and those concerned in such a strictly mental operation as the use of written language. These processes appear in this view not as two heterogeneous functions issuing from the same centre, but as naturally and closely allied, the one constituting, indeed, the very foundation of the other.

Now if this be the true view we should expect to find the same parts of the brain to be concerned in the muscular movements of articulation as are concerned in the use of language. Here experimental physiology comes to our aid; for although the lower animals do not speak, we can determine in some of them the part of their brain homologous with the convolution of Broca, which is so constantly affected in cases of aphasia. In Dr. Ferrier's latest detailed paper, quoted at the beginning of this review, he describes experiments produced by excitation of two contiguous portions of the convolutions, numbered respectively nine and ten in his diagram of the monkey's brain, "corresponding in situation to the lower part of the ascending frontal convolution, or posterior part of the inferior frontal convolution, above the lower end of the fissure of Sylvius (Broca's convolution)." Among other movements it is recorded that on stimulation of No. 9 the tongue was protruded; and that on stimulation of No. 10 the tongue was retracted. "These results," Dr. Ferrier adds, "point very definitely to a centre for the movements of the mouth and tongue, the muscles concerned in mastication and also in articulation." These experiments, so far as they go, afford strong grounds for accepting Dr. Hughlings Jackson's doctrine arrived at, as it was, on quite different grounds.

But here a serious difficulty presents itself, for in cases of aphasia with a lesion in the very situation indicated, there is often, indeed usually, no serious loss of the lingual movements.

Similar difficulties constantly present themselves in the study of the views here presented. Soon after a function is localised in a certain part of the brain, from a study of "discharging lesions" producing movement or convulsion in the part, we find cases with "destroying lesions" of the same portion of the convolutions, without any distinct paralysis of the part supposed to be under its control. This is at first very puzzling. But there is reason to believe that many movements of a bilateral character, those of the tongue for example, are controlled by centres existing on both sides of the brain, so that a unilateral lesion, such as usually exists in aphasia, does not seriously paralyse the part in question. The experiments of Ferrier seem here to point in the same direction, for he found the action on the tongue to be bilateral when the centre was stimulated on either side. In certain cases also, as already indicated, a form of compensation exists from the overlapping, as it were, of one centre by another; and the higher the centre destroyed happens to be, and the more complicated its connections, the more likely is its function to be supplemented, so that its loss is less distinctly manifested by paralysis. On the other hand, the higher and more intricately related to muscular movements such a centre is, the more extensive will be the manifestation of its deranged function when it "discharges" its force in an abnormal way. This difference is only what we might expect in any elaborate organization containing different departments which, although co-ordinated and under the regulation of a head, are yet possessed of such a degree of administrative independence, that even the subdivisions of these departments are capable of dealing with matters of routine as they arise. If, for example, one or two of the higher officials of the Post Office were suddenly and completely laid aside from action, but little obvious disturbance would result to the service from such an accident; whereas, if one or two upper officials in a country branch were thus suddenly laid aside, the business depending on them would be more or less completely paralysed. Thus we see the influence of "destroying lesions" in high and low centres respectively. But if, instead of being completely laid aside, one or two among the higher officials had their sound sense and equilibrium disturbed, so that their minds became unequal to the proper transmission of orders from those above them and incapable of appreciating the varying influences at work in the country; or if, perhaps, passing from the control of their superiors they performed their duties in an exaggerated and aberrant manner, such a proceeding might lead to results fairly to be described as a convulsion in the service, affecting probably every department of the business, and reaching in its results

to the... Such aberration, on... in a small country office... part of the postal business, and that probably only in a very slight and limited manner owing to the comparative simplicity of the transactions involved. We might substitute for this simple comparison the more elaborate organisation of an army, having not only its different branches of cavalry, infantry, and artillery, but also those elaborate departments concerned in the supply of ammunition, food, medical aid, and the prompt transmission of intelligence, which permeate and affect every branch of the service; such a comparison might serve to bring out in relief the great difference in "destroying" and "discharging" lesions, according as they affect the higher or lower centres, or as their influence extends to the vital processes governing the very life of the whole.

And it is of great importance, for the understanding of the doctrine in its bearing on insanity and epilepsy, that the relation of these highest centres should be grasped. The highest centres are supposed to be those which have the most varied and most comprehensive relationship with all those lower centres which preside over the sensory and motor operations going on throughout the body, not forgetting the more occult processes connected with the viscera and blood-vessels which have such an important influence on the whole economy. Dr. Jackson here adopts the views and employs the language of Herbert Spencer in announcing his opinions. According to both "the seat of consciousness is that nervous centre to which mediately or immediately the most *heterogeneous* impressions are brought," so that these highest centres in the convolutions of which we are speaking, *because* of their multiform relations, are to be regarded as the "anatomical substrata of consciousness;" in proportion as these are disordered, consciousness becomes... or wholly lost; degrees of unconsciousness, ... be recognised in proportion as there is in the ... "defective correspondence with the environment."

Now, if a sudden paroxysmal discharge occurs in the convolutions, and if it begins in these highest centres, we are prepared by this hypothesis to expect a sudden disturbance of consciousness, a general and almost simultaneous perturbation of the various muscles throughout the body, and some affection of the visceral and vascular system. In the pallor, the general convulsions, and the unconsciousness of a typical attack of epilepsy, we have all these affections in a marked form. If now we

discharge to begin in a lower centre, although no ... a high one, viz. that connected with visual impressions, we may have a fit ushered in by the sensation of

great masses of colour, before consciousness becomes abolished, and before the general convulsions supervene. If even a lower and simpler centre be the starting-point of the discharge, we may have the finger or the arm convulsed to a marked degree before consciousness is lost (or perhaps without any loss of consciousness at all); the point of time at which unconsciousness occurs in such cases often coincides with that at which certain further groups of muscles begin to partake in the convulsion, indicating the spread of the discharge to further and probably higher centres in the brain. Or, perhaps from some peculiarity in the position or the amount of the "discharge," we may have only the transitory loss of consciousness, without convulsion, observed in *petit mal*; or it may be that a sudden pallor is all that can be alleged. Some of these points have had an interesting light thrown upon them by Ferrier's experiments. He found on stimulating a particular part of the middle frontal convolution in a monkey, that the hand was raised towards the mouth, and that, on further excitement of this centre, the hand was not only brought to the mouth, but that the angle of the mouth itself was retracted; the proper centre for this movement of the mouth, however, was found by experiment to be immediately below the other; we have here, then, a stimulation of one centre propagating itself to another, and selecting, as it were, one with a function fitting in as an appropriate addition to the other's action. Prolonged irritation of any part of the convolutions was often found to give rise in the animals to general choreic movements and even to universal epileptic convulsions.

Dr. Hughlings Jackson has studied with great care this sequence of allied movements in the physiological or healthy exercise of muscles as related to the sequence observed in the invasion of different parts in a convulsive fit; he has shown that in lifting an object with the right hand, for example, we call into action, according to the weight encountered, first one group of muscles in the limb and then another, till those of the trunk itself become involved, and even the limbs on the opposite side begin ultimately to participate in the effort, if this be considerable; and so in a convulsion, according to its degree of severity, the action may pass from one group of muscles to another, and may even cross to the opposite side of the body from that on which it began, and to which by the anatomical connections of the lesion in the brain it was directly related.

Another very interesting point in connection with the "discharges" or convulsions affecting the higher centres is brought out by referring to some of their after-effects. Convulsions limited to one side of the body are well known to be frequently associated with paralysis limited to the same parts: this, when

permanent, is no doubt often due to some lesion of the motor tract on the same side of the brain as that on which the unstable centres have exploded in paroxysmal discharges; but when the paralysis is more or less transient, as is often the case in epileptic hemiplegia, we may well refer the loss of power to an exhaustion of the centres from their excessive discharge of force; in such a case this exhaustion affects their motor function. But if some of the highest centres be involved, we might expect as they are the "anatomical substrata of consciousness," that, this exhaustion after violent convulsions would also at times manifest itself by some evidence of a "defective correspondence with the environment;" and so we find in the records of epileptic mania all sorts of aberrations of the mind, many of them quite compatible with the automatic carrying out of even complex processes; and these may go on quite apart from the true controlling consciousness, which, if it had been intact, would have prevented them from running at one time into grotesque absurdity, and at another into harrowing tragedy. Such temporary paralysis of the highest centres finds an analogue in the occasional transitory blindness for the particular colour whose glare had constituted the premonitory aura of an epileptic fit.

In this exposition of the relation of the doctrines of cerebral physiology to the study of epilepsy we have referred only to two distinguished workers in this country, and their labours have, on the whole, been the most important and the most fruitful; the inquiry, however, has found ardent students in other countries. Ferrier, indeed, borrowed the idea of his method of stimulating the cerebral convolutions from Fritsch and Hitzig, who had made some advance in the localisation of movements in the brain. From the clinical side, the doctrines of Hughlings Jackson have found enthusiastic disciples in Charcot and his pupils, and the treatise by M. Landouzy at the head of this review is a valuable contribution to this department of the subject;—the epithet "Jacksonian" is even being introduced in France by Charcot to be applied to epilepsy or convulsions of a localised and partial character.

The same line of inquiry is being pursued by M. Luys by yet another method, and although the results must be received, as in every new and complicated method, with much caution and reserve, the indications arrived at seem to be somewhat similar to those drawn from previous studies. It has long been known that when nerves are cut, or limbs completely paralysed, there is an atrophy gradually developed in those cells of the spinal cord with which such nerves are directly connected, and the method of experimental injury suggested by this fact has already been employed in the study of certain questions in physiology and patho-

logy. It is conceivable, therefore, that if the convolutions of the brain be the real source of muscular movements, these should be found to have become atrophied when patients have lived for many years without the possibility of exercising the functions concerned; as in the case of those whose limbs have been amputated, or whose power of speech has been suppressed through deafness. The four specimens recently presented by M. Luys to the Society of Biology,¹ indicate the necessity of following up this method of inquiry with the utmost care.

The importance of these hypotheses, in connection with the pathology and treatment of epilepsy, can scarcely be overrated. No doubt the first results will be in the direction of supplying a scientific classification of epilepsy and convulsions, rather than of furnishing much practical assistance in the treatment of those affections. But such a scientific classification will afford great advantages in pursuing the study of the causes which lead to explosion of the nervous centres, and of the processes by which explosion is brought about. We are glad to find, however, that in thus seeking earnestly for a scientific classification of epilepsy Dr. Hughlings Jackson no longer treats as idle or useless those great clinical distinctions on which the differentiation of "fits" has hitherto proceeded; much additional light must yet be gained before the classifications or groupings in present use can with any propriety be set aside. Even now, however, there are gleams of hope that this new pathology of epilepsy may soon assist in the proper appreciation at least of those dietetic and medicinal measures which have been found useful in the treatment of this formidable affection.

V.—On Certain Endemic Skin Diseases.¹

DISEASES of the integumentary system have occupied a very prominent position in medicine from the earliest historical periods, for we find that reference is made in the Old Testament and other ancient works to many cutaneous affections;

¹ "Modifications survenues dans l'état de l'écorce cérébrale par suite de la disparition de différentes catégories d'incitations périphériques," par M. le Dr. Luys, 'Gazette des Hôpitaux,' July 11, 1876, p. 637.

² 1. *On Certain Endemic Skin and other Diseases of India and Hot Climates generally.* By Drs. FOX and FARQUHAR. London, 1876.

2. *Chinese Customs, Medical Reports for the Half-year ending September, 1875.* Shanghai, 1876.

3. *The Fungus-disease of India; a Report of Observations.* By Drs. LEWIS and CUNNINGHAM. Calcutta, 1875.

4. *On the Psoriasis or Lepra.* By GEORGE GASKOIN. London, 1875.

but, from the imperfect descriptions given in many cases, it is rather difficult to know exactly to what skin diseases they refer.

As in every other abstruse subject influenced by innumerable and changing conditions, knowledge has been slowly progressive, but not, perhaps, to such an extent as the importance and distribution of diseases of the skin demanded. In fact, it must be admitted by every one conversant with the literature of skin affections generally that dermatology is yet in its infancy, for frequently some new investigation into the pathology of many affections of the skin dashes to pieces in an instant the theories on which medical opinions have rested for ages. In addition to increased facilities and improved methods of research, the microscope, for example, in the hands of competent observers, has done wonders in placing a knowledge of skin diseases on a more scientific and, therefore, satisfactory basis; nevertheless, there is still a great and unexplored field open to the practical investigator, who will be sure to obtain his reward.

To our continental neighbours the French, and especially the Germans, the chief credit is due of instituting searching inquiries into the morbid anatomy and histology of the skin in various diseases, and of thereby indicating the future line of research from which the best results may be anticipated. It may seem invidious to mention names, but it is impossible to do justice to the labours of Hebra and Bazin in connection with skin affections.

Perhaps no class of diseases are more influenced by temperature and climate than those of the skin, and undoubtedly the same diseases become much modified, if not somewhat changed, in different countries.

A very novel contribution to the literature of skin affections, as they occur in various places, especially hot climates, has been recently published by Drs. Fox and Farquhar.

These gentlemen, recognising the uncertainty which prevails amongst the profession as to the real nature of some of the common forms of skin disease endemic in the colonies and tropical countries, have, at the recommendation of the Army Sanitary Commissioners, prepared a short account of what was already known of each, and framed a number of questions, all of which were published in 1872 under sanction of the Government of India, and copies were distributed to army, naval, and civil medical officers serving in various quarters of the globe, with a view to elicit certain desired information. The work under consideration contains the answers and reports of the medical officers, in reply to the queries put to them, prefaced by a

critical abstract and analysis of the reports by Drs. Fox and Farquhar.

The subjects regarding which information was asked were fifteen, the most important and most common in hot climates being Frambœsia, Delhi sore, Ainhum, Elephantiasis Arabum, the Fungus foot of India, and Leprosy.

We have read through the reports of the various medical officers, and we must admit that it is difficult to cull any further knowledge in the majority of instances regarding the nature of the skin affections of which they profess to give an account, and if proof were wanted of the old saying that "doctors differ," it would be found amply illustrated in this report.

We do not, however, find much fault with the physical descriptions of the various skin affections, though in most cases they are very brief and fragmentary; but we do think that officers belonging to a scientific department should have made less use of theory and more of practice; in other words, made more use of their microscopes and other means of investigation if their reports are to be really of value.

One of the most important contributions in the work is that from the pen of Dr. Vandyke Carter, Indian army, who has given interesting accounts from actual examination of the diseases known as "*bouton de Biskra*," "*bouton de Crete*," and "*bouton d'Alep*," with five coloured drawings representing the appearance of those affections.

Very little information has been given in addition to what was known of frambœsia or yaws forty years ago, and there is no notice of any histological examination having been performed, at all events, in recent years, though the disease is very common amongst the negroes in Africa, America, and especially in the West Indies. Judging from the latest description of yaws in Dr. Tilbury Fox's work on skin diseases, it appears to us that there are some points in common with that severe affection known as "Delhi ulcer;" but until its pathological anatomy has been worked out its nature will be purely speculative.

Nothing has been added to our knowledge of "Delhi sore" as it occurs in India; but Dr. Carter has contributed accounts of allied if not identical affections, as already mentioned, during his recent travels in Crete, Italy, Algeria, &c., while engaged prosecuting researches into the nature of leprosy. He succeeded in proving to his own satisfaction, and his opinions are supported by those of other medical officers serving in Arabia, &c., that Aleppo boil or bouton, Biskra bouton, Scinde and Delhi boils, are examples of the same disease.

He has also made the discovery that, in addition to the cell growth first discovered and described by Fleming ('Army Me-

dical Reports' for 1869) as constituting the principal histological feature in the disease in India, there is in Biskra bouton "a true vegetable organism (including a distinct mycelium) within the lymphatic vessels in and around the clou (or bouton) at a certain stage of development." Hence, he adds, "a clue to the unravelling of many mysteries concerning this remarkable skin affection, which otherwise seem quite inexplicable."

This is a very important discovery, because it will assist to confirm the view that the disease is local, and that it should be treated by local remedies, as recommended many years ago, and now practised successfully, as regards Aden ulcer, Mooltan and Delhi sore, Scinde boil, &c., since these diseases are of the same nature.

Carter proposes for the disease the name "mycosis cutis chronica," now that he has discovered a fungus; but it would be well before accepting it to wait for the results of researches which we understand have been commenced in India long since by Drs. Lewis and Cunningham, regarding the Delhi sore. If a fungus is present, and if it is the cause of these troublesome contagious affections, we hope it will be readily detected; though many authorities deny that fungi have any share in the production of disease, and when they are observed associated with morbid states look on them as secondary, or as the result of diseased conditions.

We should expect, however, if the cause of Delhi sore and its allies be traced by further investigation beyond a contagious cell growth to a fungus with mycelia in the lymphatic vessels, that the lymphatic glands, at all events near the sore, as well as the lymphatic vessels themselves, would show some signs of irritation. On the contrary, we know from experience that such is not the case, for there are no outward appearances or constitutional disturbances to lead us to believe that in such diseases the lymphatic system was affected in any way.

Other microscopists have not been so successful as Dr. Carter, for we learn that two sections of the Biskra bouton from early and advanced cases were examined by Drs. Fox and Farquhar, but they failed to detect any fungoid elements in the structure of the bouton, excepting some common mycelial filaments in the superficial cuticular layers, evidently accidental.

Further, we know that sections of the Delhi boil, in every stage of the disease and under the effects of various reagents, have been examined by high powers without detecting mycelia or other fungoid elements permeating the tissues; yet it is quite possible that they may have escaped notice.

It is surprising with what tenacity some writers stick to the theory that Delhi sore and its allies are the local manifestations of a constitutional disease, because they cannot, by any process

of reasoning, bring their minds to comprehend the possibility of the disease being due to purely local causes; but one writer (Dr. Geber) goes farther, and after a lengthened practical examination of Aleppo evil, during a residence at Aleppo, he has come to the conclusion that, in the majority of cases, it is a syphilitic, lupoid, or eczematous disease!

No one will be disposed to deny that syphilitic disease may coexist with and modify these affections, and may make them difficult to diagnose; but the history of the cases and the appearances will, in the majority of instances, be sufficient to distinguish them.

In persons suffering from Delhi sores we are certain there is no special cachexia; they affect the strong equally with the weak, and there is no apparent constitutional disturbance either before or after their appearance. Further, the cicatrix resulting from a Delhi or Mooltan sore is, in almost all cases, smooth, not depressed, and particularly white; whereas that from a syphilitic sore is depressed, rough, and of a brownish or dark colour, and the cachexia is well marked.

The report on Delhi sore contains many statements to which we take exception, notably those of the Army Sanitary Commission, who, in their official report for 1873 and 1874, attribute, we think, without sufficient data, the diminution of the disease in Delhi (presumably also amongst the European troops) to the improved sanitary condition of the town compared with former years. They say: "This improvement was attributed to the ameliorated sanitary condition of the town; the drainage and sewerage had been cleansed, flushed, and improved, and the city itself and the water supply had been purified."

The Army Sanitary Commission seem to have forgotten that since 1869 or 1870, when the contagious nature of the disease was proved satisfactorily, and, as one of the results of such discovery, a line of treatment was adopted to prevent the disease spreading, it has been to a great extent under the control of the medical officer, and the consequence has been that it has very considerably diminished, since open sores which propagated the disease have been easily cured, and on the same principle there is no apparent reason why it should not be entirely got rid of.

We are sorry that the Army Sanitary Commission are not more particular and inquisitive as to why there is an increase of the disease during some years and a diminution during others, as we cannot accept their explanation which may be true regarding the cause of diminution of zymotic diseases; but we venture to predict that they will eventually agree with us that the diminution has been owing to measures which prevent the disease spreading, viz. early treatment by local and destructive applications,

as potassa fusa and strong mineral acids, methods for prevention of contagion from open sores, prevention of over-crowding, &c.

In India there is no want of subjects for investigation, especially in the department of skin diseases, many of which are unheard of in this country, but amongst others a very curious malady called *ainhum* has recently been discovered in India, hitherto thought to have been entirely confined to Africa. The disease consists apparently of hypertrophy of the skin of the little toe, the bones of which become converted into fibrous tissue, and, by some process of constriction in the skin, the parts are spontaneously amputated.

Nothing has of late been added to our existing knowledge of leprosy as it occurs in India, with the exception of what is contained in Carter's work, published in 1874. Dr. Carter studied leprosy very carefully, and the conditions under which it exists in Norway, and found from statistics that, owing to various causes, the disease, at one time very prevalent there, is now declining rapidly, and consequently he has made suggestions for the treatment and prevention of leprosy in India, on a similar plan to that which has been so successful in Norway.

The Indian Government authorities are, however, evidently not in accord with Carter's views, if we are to judge from the answer of the Under Secretary of State for India to a question asked lately on the subject in the House of Commons, and from which it appears that a further inquiry by others has been directed to be made in India, with a view of ascertaining "the correctness of the conclusions at which Dr. Carter had arrived."

What the result of the new inquiry which is now going on may be we know not; but it appears to us there can be no question of Carter's great ability for clinical and microscopical research, and it would be a pity if professional jealousy should, were it only for one instant, interfere with any inquiry tending to the elucidation and treatment of one of the greatest plagues of the human race.

A description by various medical officers of one of the common and at the same time most formidable of skin diseases met with in most parts of India, viz. Elephantiasis Arabum, occupies a great many pages of the report. This disease, known from remote antiquity and described for years past in most text-books of skin diseases, is widely distributed over the world; but it is only very recently that its pathological anatomy has been worked out.

Sometimes associated with this an enlarged condition of the scrotum exists, from vesicles on the surface of which a milky exudation takes place, and with regard to this milky exudation a very suggestive discovery has been made by Dr. Lewis,

Army Medical Department, viz. that in some cases the milky exudation contains multitudes of minute living filariæ. It has consequent on this discovery been suggested that elephantiasis, lymph scrotum, and other symptoms of obstruction in the lymphatic system, may be owing in some way to these filariæ; but the question is still *sub judice*, and it is a very interesting one.

We have not yet heard that the locality of the parent worm, or the worms themselves that produce the filariæ have been discovered; but surely many opportunities exist in India for post-mortem examinations of the lymphatic glands and vessels of persons suffering from elephantiasis, chyluria, or other affections of the lymphatic system, in whom during life filariæ were found. In such cases a careful microscopical examination would contribute to settle the question, and this we understand has been done in two instances, but without throwing any light on the subject.

2. In the 'Chinese Customs Gazette' for the half year ending September, 1875, there are some remarks on lymph scrotum, elephantiasis, and chyluria by Dr. Manson, who it appears independently discovered the existence of lymph scrotum in China, and designated it as such, though the pathological condition was known since 1854. Dr. Manson believes that the three diseases are intimately connected and are owing to the same causes, viz. the filariæ discovered by Lewis; and he takes some trouble to prove this, but we do not think he has succeeded. These reports, published by order of the Inspector-General of Customs in China, are of great value, inasmuch as they contain a variety of information on various subjects, compiled by medical officers serving in remote parts of China. The present one contains, in addition to the articles already referred to, reports on the health of Amoy, Foochow, Hankow, and Shanghai, and also a favorable report on the use of the infusion of the root-bark of *Ailanthus* in cases of dysentery. We may observe that the gum resin which exudes from the incised bark of *Ailanthus Malabarica*, a plant of the natural order *Simarubaceæ*, has been for years used by the natives of India in the treatment of dysentery.

No. 3. *Fungus foot of India*.—Nothing, as far as we can make out from this report of Drs. Fox and Farquhar, has been added to our knowledge of the fungus foot of India; and so far as the special report by Drs. Lewis and Cunningham goes, it tends rather to disprove its parasitic origin.

All investigators, however, agree that in the dark variety of *Mycetoma* a fungus does exist, but in the pale variety it is still very doubtful, and it is strange that even at the present day

when means of diagnosis have been so perfected, that there should be any difficulty in determining whether a structure belongs to the animal or vegetable kingdom; for instance, doubts exist as regards the composition of the fish-roe-like particles of the pale variety of *Mycetoma*. Carter believes they are fungi in a state of alteration, but other able microscopists, including Professor Cohn, Dr. Bristowe, Dr. Fox, Dr. Lewis, &c., believe that they are of a fatty nature.

Drs. Lewis and Cunningham have apparently taken a great deal of trouble in investigating the histology of fungus foot, and they have performed numbers of carefully conducted cultivation experiments with the different diseased structures, as well as with the fish-roe-like and black particles of both varieties, with a view to develop the parent mould (*Chionophye Carteri*), but the results were negative in all cases, since nothing but the commonest of moulds were developed. Indeed, Drs. Farquhar and Fox, judging from Carter's figures, consider that he (Carter) has mistaken an *oidium* produced from the cultivation of the fish-roe-like particles of rice paste for his *Chionophye*.

It must be acknowledged that microscopical investigations with high powers are attended with great difficulty, and that some persons would be liable to misinterpret or to make mistakes as to the nature of what they see; for example, few persons are aware that a peculiar fatty substance met with in healthy and diseased tissues, and termed by Virchow "*Myeline*," will, under the influence of Liquor Potassa, assume forms very much resembling the mycelia and spores of fungi. Lewis and Cunningham give an illustration of this, produced in the examinations of a fish-roe-like particle, and which bears a wonderful likeness to fungi; but a practised observer in the course of a continuous set of experiments could hardly be mistaken as to their nature. We are not at all satisfied as yet, notwithstanding all the reports and authorities to the contrary, that the disease is not owing to a fungus, because there are so many vital points connected with it quite unexplained; but we trust that carefully conducted microscopical and clinical investigation may be pursued a little longer before accepting the conclusions arrived at by Drs. Lewis and Cunningham, viz. "That taking everything into consideration it seems probable to us that some local degeneration takes place in the madura disease, giving rise to a product which is in one of its varieties, peculiarly adapted to the development of vegetable organisms."

4. *On Psoriasis or Lepra*.—In reverting again to the subject of skin diseases it is a matter for congratulation that in this country we are comparatively free from many of those scourges which are common in tropical and other countries. We have,

however, widely distributed amongst us, and known for ages, a disease of the skin, considered to be hereditary, whose nature is still very obscure, viz. psoriasis or lepra, and which has evidently been confounded with the leprosy of the Old Testament. We think with Hebra that the term *lepra* should be confined to the true leprosy, or that it should be synonymous, as it is otherwise liable to lead to confusion. Physicians who have been in practice, even for a short time, must have met with cases of psoriasis, as it is almost as common as eczema, and which doubtless will have tried all their patience and ingenuity to effect a cure.

The literature of the subject is very extensive, and in all works on skin diseases it is treated of more or less fully; but perhaps one of the most complete monographs on psoriasis, that we have seen of late is by Dr. Gaskoin, a physician of great practical experience in the treatment of skin affections.

The book is divided into ten chapters, which deal minutely with the causes, affinities, varieties, pathology, and treatment; to which are added a great number of useful formulæ of prescriptions and a copious index.

The supposed exciting causes of psoriasis referred to by patients are sometimes quite fanciful. In men, however, it is found associated in the majority of cases with phthisis, asthma, gout, and rheumatism, and in women, in addition to the above, with some derangement of the reproductive organs. Dr. Erasmus Wilson, one of our most celebrated dermatologists, considers that psoriasis is "a manifestation of the syphilitic poison after transmission, through at least one, and probably through several generations."

The pathology of the diseased process has not been thoroughly worked out, and recent investigations tend to indicate that the nervous system generally or, when the disease is localised in patches, that the nerves of the part are in some way implicated.

A good deal of light has been thrown on the subject by the investigations of Gustav Simon, Hebra, Werkeim, &c., who find that, in addition to the well-known appearances of the epidermic scales, the skin beneath is in a state of hyperæmia or chronic inflammation, the papillæ enlarged and very vascular; and Werkeim's conclusion is, that the patches of psoriasis are produced by impeded circulation in the skin of the part.

Dr. Neumann, of Vienna, has carried his investigations a little farther, and, in addition to the above features, finds a cell growth which is most abundant along the course of the vessels of the papillæ. A cell growth, however, is not uncommon in many other affections in which there is chronic inflammation of the skin, and is by no means peculiar to psoriasis. The physi-

cian has often the greatest difficulty in distinguishing between psoriasis and syphilis or a combination of the two, in cases where there is no syphilitic history, and accordingly various symptoms and signs are relied on to point out the differences.

Under this head Dr. Gaskoin insists on the well-known tendency of syphilis to destroy the soft parts, or cause a breach of surface, also its ubiquity, the form of the eruption, &c., all of which distinguish it from psoriasis. Anderson in his work on psoriasis admits the great difficulty of forming a correct diagnosis in such cases, and he gives a long account of the points to be attended to in doing so; while Neumann is satisfied with the appearances presented by the scales, which he says in psoriasis "are large scaly masses of a lustrous pearly appearance (from the presence of air between the epidermic lamellæ) loosely attached to the substratum, on the removal of which a bleeding surface is exposed;" in the second (or syphilitic form) "the scaly masses are minute, of a dirty whitish colour, closely adherent, and on removal showing a pale red infiltration; in the latter case the scales originate merely in an exfoliation of the superficial layers of a syphilitic infiltration."

In a short paper published in the 'Medical Press and Circular' of the 26th of April, Dr. Myrtle gives the following symptoms and signs for diagnosis:

"1st. The fact of previous syphilitic poisoning.

"2nd. The presence of febrile symptoms.

"3rd. The character of the eruption, which in the syphilitic form generally appears most abundantly in those parts of the skin least affected in the true form—the insides of the extremities, palms of the hands, and soles of the feet.

"The scales are smaller, less adherent and vary in shape." "The colour of the hyperæmic patches is of a dusky brown and on examining the mucous membrane of the mouth you will generally find patches on the tongue and throat with fissures." "These I have never found in true psoriasis." "Then we have the action of remedies to assist our diagnosis, &c."

That part of Dr. Gaskoin's book devoted to treatment is as complete as can be, and contains the latest information collected from various sources; but considering that the causes of the disease are obscure and that its pathology is still misunderstood, the treatment as might be expected is rather empirical. Cases from practice illustrative of the various phases of the disease under all conditions are given throughout the book, which is altogether a safe guide for the treatment of a very common affection.

The work is very pleasantly and humorously written, and we have no hesitation in recommending it, since it contains all that is known on the subject of psoriasis.

VI.—Pathology of Pneumonia.¹

JUST about one hundred years ago Cullen settled, to his own satisfaction, the proper treatment of pneumonia upon what he no doubt believed to be the irrefragable basis of a sound pathology.

For seventy odd years Cullen's opinions—shall we say, or merely the practice that had grown out of them—so strongly swayed both the professional and the public mind that not only pneumonia, not only inflammatory disease, but all disease, of whatsoever kind, in which from any cause whatever an inflammatory tendency was believed to exist, was invariably treated by the inevitable bleeding; nay, more, so strong was the faith reposed in this treatment that some physicians, with very considerable pretensions to talent and clear-headedness, rather than give up their faith in its efficacy, were forced to accept the other horn of the dilemma presented to them and put an equal trust—which soon grew to be preferential—in the decillionth of a grain of phosphorus and similar transcendental remedies. While still others, some few of whom yet survive, unable to deny that pneumonia is recovered from under inert or simply dietetic treatment, and yet unwilling to confess that the little puddles of blood which bulk so largely in medical history were shed in vain, have sought for an explanation of the fact in some fancied change in what is called the type of the disease. Altogether forgetting that Cullen bled so freely, not because the type of the disease was more sthenic then than it is now, as his own MS. clinical lectures clearly prove, but because his pathology taught him that depletion was a rational mode of attempting to cure a disease the essence of which he believed to be spasm of the pulmonary arterioles, and the most frequent cause of death in which morbid anatomy taught him to be rupture of the blood-vessels and escape of the blood into the pulmonary tissue. Upon one occasion, indeed, he acknowledges that he fell into a mistake which is said occasionally to happen now-a-days; he treated a patient for continued fever who was afterwards found to have died of pneumonia, and the most important remark he makes to his class is an expression of regret that he had not recognised the disease sooner, as in that case he should most certainly have bled the patient and he believed with benefit.

¹ 1. *The Natural History and Relations of Pneumonia; a Clinical Study.* By OCTAVIUS STURGES, M.D., F.R.C.P. London, 1876, pp. 336.

2. *Croupous Pneumonia.* By THEODORE JUERGENSEN, Professor of Medicine at Tübingen. ('Ziemssen's Cyclopædia of the Practice of Medicine,' vol. v.)

Even in Cullen's day there were many individuals who as he says were "by their constitution ready to faint upon a very small bleeding" (First Lines, 7th edit., p. 332), but he bids his reader take heart, for though they might be unable to abstract as much blood at first as "a pneumonic inflammation might require," these very patients, he says, will bear after bleedings much better than the first, so that by care and gentle suasion "as much blood may be taken away as the symptoms may seem to demand."

It is evident that the absence of sthenic symptoms was not accepted by Cullen as any indication of a change of type of the pneumonia,¹ nor was even the presence of what is usually looked upon as one of the most evident indications of asthenia at all regarded by him as any proof that active medication was likely to be either hurtful or superfluous. For indeed what possible change of type could alter the causal relation of spasm to congestion, if it existed at all, or materially influence the effect of blood-letting upon either spasm or congestion? Cullen's treatment was clearly the legitimate inference from his pathology; any facts, therefore, which tend to prove his treatment to have been unnecessary must be accepted as still more clearly proving the incorrectness of the pathology upon which it was based.

To prove this, however, is to create a revolution in medical science, a very different matter indeed from making a mere change in medical practice. A change in practice is being made every day from mere individual caprice; a revolution in medical science has never before occurred, and none so great or so complete is ever likely to happen again.

A single glance backwards will speedily convince us of this.

Whether we owe it to Podalirius, or to Pliny's Hippopotamus, the introduction of phlebotomy was undoubtedly a most important change in medical practice, but we may safely acquit either the one or the other of these sponsors of having the very slenderest knowledge of medical science, the one from the evidence of the recorded facts, and the other for very obvious reasons. But phlebotomy is a treatment possessed of unquestionable power; we may not be able to cure our patients by it, but we can always produce a decided effect; we can always cause them to faint, and the physiological properties of syncope are such that there are no diseases the symptoms of which are not modified on its occurrence. These symptoms may suddenly cease

¹ In the 'Lancet' for Dec. 9, 1876, there is a letter at p. 839 which distinctly proves this on the authority of Cullen's own MS. notes. For it is there shown that it was in Cullen's days no uncommon thing for patients to faint on the abstraction of only four ounces of blood, yet there was no difficulty in those days of pushing the bloodletting as far as was thought necessary, even in the case of such patients.

never to recur; they may temporarily cease only to recur; or in rarer instances they may be aggravated, but they never remain unchanged. Treatment of this character shows power, and, whatever happens to the patient, it is always of advantage to the practitioner in a rude age or among a savage or even a merely ignorant people. "What devil is in this salt?" said an Affghan physician to Ferrier, showing him some cyanide of mercury, "for of a hundred patients to whom I have given it only one has survived." Such treatment, however, did him no harm in a region where weakness is the only unpardonable sin.

Apart altogether, therefore, from the incitement supplied by the immense rewards¹ with which the first recorded successful exercise of phlebotomy was crowned, we cannot wonder at the extraordinary attraction so powerful a treatment had for both the profession and the public. And yet what science there was in those early days was opposed to this heroic practice, and from Pythagoras, who lived 500 years before Christ, and was the earliest medical philosopher, and Erasistratus, Aristotle's grandson, whose writings and example largely swayed medical opinion, down to the second century of our era, it was continually protested against as a needless waste of life, and especially because the abstraction of this fluid life only served to diminish the strength of the patient without affecting that coagulation of the blood which was apt to occur in and around the extremities of the veins and arteries as the result of any general disturbance of the system, which interfered with the movements of the *pneuma* or vital spirit, produced a perturbed action of the heart and arteries and constituted what was called an inflammation. The removal of this coagulum, Erasistratus taught, could be effected and ought to be entrusted to the power of nature alone aided by diet. It is remarkable, considering the period when they were enunciated, how little these views vary from those which have been promulgated quite in our day, for hear what the late Dr. Bennett has said:

"Pneumonia consists of an exudation into the vesicles and tissues of the lung, which coagulates and excludes the air. It is very doubtful whether a large bleeding from the arm can operate upon the stagnant blood in the pulmonary capillaries—that it can directly affect the coagulated exudation is impossible. But lowering the strength and vital power of the individual is directly opposed to the necessary vital changes which the exudation must undergo by cell-growth and disintegration."

At the epoch we have mentioned, towards the latter end of the second century of our era, Galen, the most brilliant special

¹ The hand of Syma, and for dowry the Carian peninsula.

pleader that medicine has ever produced, directed a series of caustic diatribes against Erasistratus who had been dead for 400 years, and against his followers who were then the most distinguished practitioners at Rome. Galen's eloquence turned the tide; forced at first to flee from Rome he speedily returned triumphant, and by the force of imperial favour and the *al captandum* nature of his arguments he succeeded in so finally establishing phlebotomy as the rule of practice in inflammation that henceforward it continued to be pre-eminently orthodox. And though Erasistratus never ceased to have distinguished followers in every age, yet the discovery of the circulation, after the rebirth of science which followed the dark ages, seemed to lend irresistible force to the arguments of Galen. And when towards the end of the 17th century the researches of Bonetus into human morbid anatomy began to confirm the statement first made by Lælius à fonte as to the hepatisation of the lungs in pneumonia, when by-and-bye the researches of Valsalva, Morgagni, and Lieutaud, proved that this hepatisation was invariable in all those who died with symptoms of pneumonia, and when Cullen, a special pleader perhaps only second to Galen himself, skilfully or wilfully ignoring all that had preceded him, except what suited himself, promulgated his doctrine as to this hepatisation being produced by the escape of blood from a vessel ruptured from excessive congestion due to spasm of the pulmonary arterioles (Hoffman), a congestion which could only be successfully combated by free and copious venesection, then indeed the treatment of pneumonia by phlebotomy seemed to be placed for the first time and for all time upon an indisputable scientific basis. So admirably was the net woven, and so great was the immediate relief given by this treatment, that it seems probable that most of us might be practising it yet had not Hahnemann and Skoda given us practical proof that infinitesimal doses of phosphorus, or fractional doses of extractum graminis, corrosive sublimate, or opium, were equal in efficacy to the most powerful venesection. This *reductio ad absurdum* has fortunately been sufficient to turn the scale in favour of the old Pythagorean doctrine, and even the vigorous attempts made to prove that the accepted failure of phlebotomy is due to a change of type in the disease, have only resulted in a momentary and futile haze which temporarily obscured the position of the combatants, but which is now fast clearing away. Not the least singular fact connected with this history is, that the accepted failure of phlebotomy, as a curative agent in pneumonia, was never recognised until it was forced upon its advocates as the only alternative to recognising the active agency of infinitesimal medication. This very fact we think ought of itself to throw doubt upon the fancied efficacy

of phlebotomy in pneumonia of any type, but it is needless to argue that point now-a-days.

It is interesting to note that, while the overthrow of phlebotomy as a system of treatment was strictly due to what—with all its pretentious display of fallacious reasoning—we cannot but call the rudest empiricism, the future revival was being at the same time made more and more difficult by the complete overthrow of that scientific basis upon which Cullen seemed to have so securely based it, from the gradual spreading among the profession of more minute and accurate knowledge of morbid anatomy, from the careful experimental study of morbid processes during life, and from the strangely altered complexion of our pathological views which has been the result of these investigations.

The first great assault on the solidism of Cullen's pathology was made about thirty years ago, nearly simultaneously with the great and final onslaught on his practice, and consisted in the return to Humoralism by the Vienna school, who regarded the hyperinotic condition of the blood as the most important element in the production of pneumonia. In this form of humoralism the fibrinous crasis was the primary disease, while its localisation as an exudation in any organ was a mere secondary affair, so that though an inflammation might run its course in the blood, just as a typhus exanthematicus did, yet, as in typhus abdominalis, an exudation took place into certain glands, so in local inflammations the crasis localised itself as an exudation into the organ affected. While in Cullen's days, therefore, the "rubor" of the ancients—the vascular congestion—attracted most observance, it was the "tumour"—the exudation—to which the Vienna school directed their attention. By-and-bye, in the Neuro-pathological school of which Henlé may be considered the founder, the "dolor" came to be regarded by many as the primary and most important alteration in inflammation. But the neuro-pathological school was essentially humoralistic in its tendencies, the difference between it and the Vienna school consisting mainly in the mode in which the exudation was brought about. As the latter school taught that the blood crasis produced that vascular paralysis which originated the stasis and favoured the exudation, while the former school utterly ignored any pre-existing crasis, and hence formed a distinct, transitional stage to the solidism of modern histologists. For just as the theory of the development of the exudation of the earlier microscopists naturally ranged itself, and to some extent originated, in the Crasen-lehre of the Vienna school, so Virchow's cell-pathology, and the more recent doctrine of the emigration of the white corpuscles,

which we owe to Cohnheim, equally naturally connect themselves with neuro-pathology. But what a vast change in pathology since Cullen's days; with the development of the Crasen-lehre it became difficult to see what good phlebotomy could do in inflammation; but with Cohnheim's views it is difficult to see how we can avoid doing harm, as the white corpuscles are sure to be increased in number by it so long as the organism maintains its power of recuperation, while their local migration cannot be affected by bloodletting. But the most recent development of pathology, as exhibited in the two treatises placed at the head of this article, is even more thoroughly opposed to any reversion to Cullen's doctrines, inasmuch as by it the local manifestations in pneumonia are entirely subordinated to the general phenomena which are regarded as of an infective character. So that a disease which has hitherto been for the most part looked upon as the most exquisite example of a local inflammation, must, from this point of view, be now regarded as really febrile in its character, and as occupying the borderland between true inflammations and true (infectious) fevers, being in every way more nearly allied to the latter than the former.

In this, its latest phase, pathology has taken a long stride backwards, and placed us once again in hippocratic times, when the first of Galen's "quatuor notæ," the "calor," bulked so largely as a prime indication of the existence of inflammation. It is the old story over again: "Immer die alte Fabel, immer nur neue Figuren;" and it is difficult to see how it could be otherwise so long as the phenomena remain the same. In one age most attention is directed to one prominent phenomenon round which the rest are made to revolve, in a succeeding age some other phenomenon is made central, and so it goes on, while we wait for that Newton who shall definitely settle the orderly sequence of events in morbid processes. In the meantime it is interesting to note how readily this doctrine fits in with the great humoral pathology which has prevailed more or less in all ages, how it seems the natural outcome of the doctrines of the Vienna school, and almost an anachronism to broach it as a novelty now-a-days. But, indeed, the idea conveyed in these treatises is not a novel one; it is nearly twenty years since Wilhelm Ziemssen evolved the same idea in the 'Prager Vierteljahrschrift,' and since then it has always been more or less present to the professional mind. Juergensen bases his views largely on Ziemssen's researches. The inquiry by Sturges is more elaborate and original, and he has taken much pains to separate pneumonia from simple hypostasis, as well as from what we suppose he would call true inflammatory

affections on the one hand and from tubercle on the other; while he has given it as his idea that its true relations lie with quinsy, herpes, and rheumatism. At the same time he seems by no means certain as to "the place among diseases" this disease he has taken so much pains to isolate and describe ought to occupy; for, after stating with some precision the points in which it differs from local inflammations on the one hand, and specific fevers on the other, he goes on to say that "for its full recognition (I would not say for its essential existence) lung inflammation is necessary, but the presence of such inflammation by no means implies or secures the presence of pneumonia." It is hardly possible clearly to understand what Sturges means by this statement, but it seems to be mainly a recapitulation of what he has previously stated, that histological inflammations of the lung must be combined with a certain class of symptoms to constitute pneumonia—a statement he will find pretty generally accepted, though we may not be all possessed of sufficient acumen to detect the essential existence of pneumonia apart from its local phenomena.

The reasons which have led Sturges and Juergensen to the idea of the constitutional characters of pneumonia are very much the same; in the following notice we shall follow Juergensen. He states first of all, following Ziemssen and Hirsch, that *the geographic distribution of pneumonia is entirely different from that of catarrh and bronchitis*, the latter increasing in frequency as we advance northwards from the equator, while pneumonia remains pretty much the same in all degrees of latitude. We might concede this without prejudice, only pointing out that all that passes as catarrh, or even as bronchitis, is by no means always of inflammatory origin, and that it is very possible if we could separate true inflammatory bronchitis from bronchitis due to other causes, the discrepancy might not seem so great. But, indeed, we altogether doubt the sufficiency of our present statistics to settle any such question; in this respect it is very remarkable to find that in London in 1842 the deaths from bronchitis were 679, those from pneumonia, 3923; in 1847 the numbers were respectively 4205 and 4142, nearly equal; and in 1873, they were 9908 for bronchitis, and 4150 for pneumonia, that is to say, a proportion of about 1 to 6 in the relative mortality of the two diseases was transformed into a proportion of 2 to 1 by an alteration not of climate but of medical opinion alone. And this is proved to be due to a simple change in medical opinion as to the prevalent form of disease by the curious fact that the public die from lung diseases generally in almost exactly the same proportion as formerly—the precise proportion being 1 out of 250 formerly,

and 1 out of 221 now ; there is no increased tendency to die from diseases of the lungs generally, but the deaths are differently distributed (*vide* 'Journal of the Scottish Meteorological Society,' July, 1874-75, p. 190 ; Buchan and Mitchell's paper on the influence of the weather on mortality).

Again, it is alleged that *pneumonia is affected in its frequency* by the season of the year very differently from pleuritis and *bronchitis*. In the elaborate paper of Mr. Buchan and Dr. Arthur Mitchell, just referred to, which is based on the mortality in London during the thirty years 1845—1874, we find (at p. 210) that the curve of mortality in pneumonia is essentially the same with that from bronchitis, the only difference being that the annual range in pneumonia is considerably less. These authors also state in regard to the mortality curve from bronchitis: "This curve is constant from year to year having its maximum in the cold season of the year, and its minimum in the warm season. Its variation from year to year closely followed the variations of temperature, and are variations of degree and not of kind." And in regard to the curve for mortality from pneumonia, which they have shown to be *essentially the same* as that of bronchitis, they add, "the curve from pneumonia is steadily marked from year to year. On the other hand, these authors have shown that there is a difference between the mortality from pleurisy and that from pneumonia, and that the curve for pleurisy more resembles the curves for rheumatism and pericarditis than those for bronchitis and pneumonia. There appears, therefore, to be not the slightest reason to suppose that pneumonia and bronchitis are differently affected by the season of the year, that is, by the weather. And we are all the more emboldened to say this, that Juergensen himself has based his statements partly upon the London statistics, of which he has only employed fourteen years—1840-1854—far too short a period to give anything like a satisfactory *constant* of the mortality of any disease. We can, however, confirm the statement he has made that the mortality of pneumonia is affected differently by season from that of pleurisy, but this in nowise favours his views, for we supplement this statement by pointing out that while the curves of pneumonia and bronchitis agree together, those from pleurisy and rheumatism also resemble each other, so that this method of research confirms the general opinion as to the local origin of pneumonia, and is decidedly opposed to there being any connection between it and zymotic diseases, under which head rheumatism is placed, with which as we have seen pleurisy is closely allied. This connection between pneumonia and zymotic disease Ziemssen endeavoured to prove by means of curve projections of the mortality of

various diseases in London during the period 1840-54; amongst these, *typhus* figures prominently; but we know that prior to 1869 all fevers were clumped together; we know also that typhus and typhoid have a very different seasonal mortality, and that both of them differ very materially from that of pneumonia; we therefore know how much value to put upon any such curve projection, and how much trust to put in any man who republishes such a projection as valuable material in this year of grace 1876.

We need not, we think, pursue this branch of the inquiry any further.

Croupous pneumonia, it is however said, *cannot be produced by any of the usual causes of inflammation, however strong or weak their action.* But this is an overstrained statement; there is no doubt that foreign bodies in the lung may produce a limited pneumonia, which we can only recognise to be not croupous by its history. Moreover, the same statement applies equally to the artificial production of a hepatitis or a nephritis, which vary in degree and extent, and even in character, according to the exciting cause, but which nevertheless are always recognised as of inflammatory origin.

During the whole course of a pneumonia, it is said, there is no constant relation between the local and the febrile symptoms, nor any dependence of the one upon the other. The object of this statement and of the next,—*The resolution of the constitutional symptoms, and especially the suddenness of their disappearance, afford us an additional proof of this,*—seems to be to prove that because the pyrexia runs a course of its own, ceasing often long before the disappearance of the local pneumonia, that therefore the pyrexia is the essential symptom of the disease, and the local phenomena are merely coincident, and have no causal relation to it. But this argument is of wider application than he thinks, and would lead to such a *reductio ad absurdum* as would be involved in the theory that a pulmonary infarction was a mere coincidence, and was the result and not the cause of that rise of temperature which so commonly accompanies its formation, and eventually defervesces not a few days or weeks merely, but months if not years, before the disappearance of the local phenomena. Moreover, recent investigations into the subject of pyrexia, some of the most instructive of which are due to Juergensen himself, “afford the strongest possible evidence that increased or diminished temperature has no necessary connexion with increased or diminished production of heat” (Burdon Sanderson, ‘Public Health Reports,’ new series, No. vi, p. 38). So that an inflammation even in the older sense of increased thermogenesis may actually go on without any

thermometrical evidence of its existence, and therefore the peculiar relations of the temperature in pneumonia to the course of the local changes cannot be regarded as affording the slightest proof of the constitutional character of the disease.

Lastly, it is said that *croupous pneumonia is a disease which runs a typical course. No affection which arises from a local lesion presents a career so definitely limited in point of time as is the case with croupous pneumonia.* Well, it is a good while now since old Van Helmont enunciated the self-evident proposition that “*cuncta sublunaria quæ non sunt morti, sunt saltem termino subjecta*” (‘*Ortus Medicinæ*,’ p. 331, par. 57), so that because a disease comes to an end, that is no proof of its constitutional origin. Perfectly local lesions oftentimes run a very definite course; thus, if we so injure our thumb-nail that it is loosened, it will be as nearly as possible six months in every case before that injury is fully repaired.

Of 100 cases of pneumonia 64 are said to terminate between the fifth and the eighth day, while of the remaining 36 a certain proportion end between the second and fifth days, and a smaller proportion between the eighth and the fourteenth days. In a disease which affects so vital an organ and normally runs so short a course, the limits of duration seem therefore sufficiently diverse in individual cases completely to nullify the aphorism quoted, and to prevent any certain conclusions being based upon any fancied definite limitation in its course, especially as there is no proof that those variations which do occur have not been due to treatment—a treatment which to be effectual need not necessarily be medicinal, but may depend for its results more upon the withdrawal of *lædantia* than on any more active system of therapeutics.

So far as the treatment of pneumonia is concerned, it is interesting for us as practitioners to observe that similar conclusions as to the nature of the disease have not led Juergensen, and Sturges to similar methods—an expression by the way which Juergensen especially reprobates. In this respect Juergensen is more consistent than Sturges; he says that the conviction that we have to do with a constitutional affection with local symptoms, and not with a local disease with constitutional symptoms, ought to lead to a radical change of the indications for treatment. “*Nature cures, and the only duty of the physician is to maintain life till the cure is effected.*” The danger chiefly arises from cardiac exhaustion, and this mainly depends upon the fever; therefore it is of importance to diminish the pyrexia by cold bathing, and to stimulate freely, that we may be able to do this with impunity, and he regards the employment of *digitalis*—as an antipyretic—as only likely to be useful

when the heart's action is vigorous, while he fears its *paralysing influence* in other circumstances. Moreover, he says: "The anti-febrile action of venesection is slight and uncertain. The physician who bleeds in pneumonia on account of the fever resembles a philosopher who cuts down the fruit tree to get at the fruit."

Sturges, on the other hand, advocates bleeding to a small amount as a means of relief to the dyspnœa, even after a considerable extent of the lung has become solid (so late as the third or fourth day); while he says that treatment by the cold bath is less applicable in pneumonia than to other febrile conditions where the structural defect is less ominous, because he regards exceptionally high temperature in pneumonia as simply an indication of a necessarily fatal condition of the lung, and he sees no reason to employ cold bathing as long as the pyrexia is moderate. On the other hand, he agrees with Juergensen in the utility of stimulants, to tide over a period of depression.

These discrepancies in the practice of two physicians, otherwise so much at one as to the essential nature of the disease, show how little dependence can be placed on pathology as a guide to treatment, even in a disease so apparently simple in its character as pneumonia; we have, therefore, the less reason to regret that we cannot accept the pathology promulgated by them.

It is singular how entirely, in these works, we lose sight of those microscopic observations which not so long since were held to have settled the pathology of inflammations generally, and pneumonia in particular, most satisfactorily. The constriction of the arterioles, following a local injury, succeeded by stasis inducing a condition of the tissues more or less akin to death, and a behaviour of the blood in these vessels as if it were in contact with inert matter, the subsequent exudation of the liquor sanguinis, and to this was quite recently added the immigration of the white corpuscles.

It is curious also how completely lost sight of are the observations of Wucherer, Varrentrap, and Baumgärtner, as to the use of chloroform in pneumonia, supplemented as these are by the successful employment in modern times of its more manageable analogue chloral, which were held to owe their efficacy to the readiness with which they were able to check those nervous reactions upon which the origin and spread of inflammation were believed to depend.

And remembering all this, and how manageable chloral is when properly administered, it is strange to find Sturges stigmatising it as "too uncertain to be trusted," and asserting that

to seek to abolish pain by its use in the early stage of pneumonia is "to prefer present comfort to future safety," we venture to think that Sturges has yet as much to learn as to the use of chloral in pneumonia as Juergensen evidently has as to the use of digitalis, in spite of all that has been recently written regarding it, and to predict that a judicious use of these drugs will be found more efficacious in the relief of the symptoms in pneumonia, and not less conducive to recovery from it, than most of those remedies of which more has been heard of in recent days.

Juergensen is a practitioner all of the German type, a compound of experimental physiology largely diluted with cold water; Sturges is a thoughtful and philosophic physician, whose present volume is well worthy of careful consideration, and from whose more mature experience we may expect much good work in the advancement of scientific medicine. Meanwhile—to fire a Parthian shot—we would just suggest, is there nothing in the anatomical structure and physiological function of the lungs to account for the differences between pneumonia and the acute inflammations of other organs? And we ask this with the greater boldness, because the symptoms and course of this disease in the lower animals seem to be greatly modified by differences in structure.

VII.—Reports of Lunacy Commissioners.¹

AMONG the statistics of disease those of mental disorders are, it must be granted, the most complete we possess. The peculiar nature of those disorders, the fact that they, with comparatively few exceptions, involve the segregation of their sufferers in special institutions, and that in each division of the realm there is a specially appointed public authority to look after lunatics, watch over their interests, and keep accurate accounts of their number and movements,—are the chief circumstances to which we are indebted for the statistical completeness referred to. Amid the strivings after a registration of diseases, and with the knowledge of the fallacies attending the registration of death, it is therefore satisfactory to be able to point annually to the Reports of the Commissioners in Lunacy as exhibiting a tolerably accurate

¹ 1. *Thirtieth Report of the Commissioners in Lunacy to the Lord Chancellor, 1876.*

2. *Eighteenth Annual Report of the General Board of Commissioners in Lunacy for Scotland, 1876.*

record of the prevalence of mental disorders and of their mortality, and a less clear conception of their etiology and curability.

Year after year these reports tell of an increasing number of insane occupants of asylums and workhouses. The heavy list of insane is a gloomy calendar, and its contemplation should awaken vigorous efforts to fathom the causes of the prevalence of so much mental disease, and to suggest measures not only for the relief and comfort of the insane, but also for arresting the production of insanity. Vast, indeed, have been, and still are, the efforts made to ameliorate the condition of the insane, and numerous but less definite and direct are the means employed to cure them, but the results are far from satisfactory when viewed in reference to the arrest of the progressive growth of insanity among the people. The removal of its causes generally may be utopian, but it may be hoped that something will be devised to stem the development of the malady in the country. To realise such hope to any appreciable extent we need to know much more of the etiology of insanity than we at present do. On this subject there is as yet little real knowledge. There is a perplexing confusion among writers between its moral and physical causes, and the battle even goes on still as to whether there is an ethereal something which goes wrong, or a change in the material and tangible organisation. Happily the arguments for the occurrence of disorder in a spiritual entity are daily declining in importance before the light thrown on nervous disorders and lesions by modern physiology and pathology; and we look for the time when—although confessedly, as hitherto employed, the scalpel and microscope have failed and yet do fail to discover cerebral lesions that may satisfactorily explain disordered mental phenomena—more accurate and minute pathological research, coupled with clearer views of nerve force, of its direction and mode of action, of its perverted operation and indirect influences, and of its co-ordination and its correlation with other forces in the body, shall serve to clear up many of the mysteries that now prevail.

At all events, the general etiology of mental disorders as distinguished from pathological investigations need be pursued much more carefully. As matters are now conducted the statement of the cause which seemed to usher in the malady is, for the most part, a simple fiction of the imagination of some relative or friend, who suggests something consonant with his own ideas of what the cause should be. And when medical men essay to discover or to elucidate the causes of insanity, they are apt to do so with foregone conclusions as to the greater influence of moral or of physical causes.

What is wanted among those engaged specially in observing

and treating mental disorders, when they would dive into their general causation, is a wider survey of the external and internal conditions which, from acknowledged physiological grounds, are known to influence nerve action prejudicially, and by and through it to effect temporary or permanent structural changes in nerve-tissue, and to be attended by correlative changes in other tissues and parts of the body, either as antecedents or consequents.

If, with the vast fields for observation before them, the medical superintendents of asylums would lend their minds to solve some general questions in the etiology of insanity they would confer a real benefit on the community, by being enabled to impart definite instruction to mankind in the art of keeping sane.

To turn, however, to the Blue-books before us: they are made up of a great deal of matter of little general interest. They require some one to analyse their contents, and especially their tables, and to put into a comprehensible shape the facts deducible. We must, indeed, not be too critical with them, remembering that they are but the yearly accounts rendered of work done, and do not profess to unfold medical or physiological facts. They are of the class of statutory productions, and consequently have to meet the requirements of statutes however profitless to science many of them are.

Excepting the statistics of the movements of the insane and some general inferences, deducible from the statutory records of visits made to asylums and other receptacles for lunatics, respecting the extent and character of the provision made for general comfort and management, and the conceptions of the Commissioners as to wants to be supplied and defects to be remedied, there is little else to be found in these official documents. These remarks, indeed, apply more strongly to the Report of the English Commissioners; and, what is singular with regard to these gentlemen, they seem to hold the opinion that they can do no better service towards elucidating the many questions of interest respecting insanity and the insane than hold on in the same course of official indifference to them. At page 22 they write, that they have at no time considered it their duty to draw any but the most plain and obvious deductions from the statistics they present; nor do they deem it advisable or justifiable to offer any speculations or theories of their own based on these statistics. And they proceed to say—

“At present we do not think that the recorded experience is sufficiently extensive to warrant many very certain conclusions to be drawn from it, and the official publication of conjecture founded

on confessedly imperfect data, and therefore liable to be falsified by the event, would not, we submit, be attended by any public advantage."

Now, anything beyond what the letter of the lunacy law imposes upon them in the way of reporting may be, strictly viewed, not within the duties they are required to perform; nevertheless, to restrict themselves to the literal performance of a statutory task is, we consider, to take low grounds and an inadequate view of their functions as the administrators of an important department in the social organisation of the state. Placed, as the Commissioners are, in a position of great authority, furnished with the widest and most varied field of observation in regard to all matters bearing upon the condition, the management, and the movements of the insane; armed with power to call for information, and supplied with assistants to prepare and digest materials submitted to them, the public might surely fairly expect some attempts to be made by them to elucidate various questions respecting insanity and the insane, and especially such as fall within the boundary of social science. In fact, in bygone years they have had a higher sense of their responsibility and usefulness than they now assume to themselves; for at different times they have called for returns respecting the treatment of insanity, the use of restraint, and other matters, with the view of placing on record professional opinion regarding them, and furnishing a groundwork for general conclusions. And, moreover, they have not, as visitors of asylums, been slow to advance their own speculations and conclusions on numerous details of treatment and management. Indeed, their reports exhibit no hesitancy on their part to accept, promulgate, and recommend transient schemes and plans and theories which have arrested their attention, and secured their approval for the time.

We cannot, therefore, find an excuse for the reticence of the Commissioners to employ the vast mass of material at their disposal to throw light upon—even though they be unable to interpret—various questions, particularly those within the domain of the social aspects of insanity. It seems almost folly to urge, as is done, that their recorded experience is not sufficiently extensive to warrant conclusions being drawn from it, in face of the fact that the present is the Thirtieth Annual Report issued. Surely a period of thirty years is sufficiently long to allow facts connected with vital statistics to be accumulated and elaborated, at least so far that some conclusions might be deduced of value to the medical profession, to sociologists, and to the public at large.

We do not actually ask for speculations or theories of their

own, although even such, proceeding from authorities so experienced, would be thankfully received, notwithstanding the possible drawback that they might be hereafter falsified by events; for is not science on every side advanced by means of hypotheses submitted to the tribunal of future experience, and often cast aside as useless and false? But we do desire to get help from these eminent public officials towards the solution of such questions as the rate of accumulation of the insane; the relations of insanity to pauperism, to occupation, to rural and to urban life. We want thoroughly to have probed the causes why, when asylum building is continuously pushed forward in every county of England, the workhouses continue with an undiminished number of insane occupants, and an unceasing demand is made to institute, in connection with workhouses, a number of petty asylums under the name of lunatic wards? Lastly, to quote no more subjects of inquiry, we want to be made sure that we are on the right road in pursuing our present policy of building large asylums for the insane of all sorts, in accumulating lunatics by hundreds with slender provision for other objects than keeping them from harm and making their lives comfortable, and whether, by such policy, we are doing our best to cure the insane and prevent the apparently ever-onward overflow of chronic lunacy?

It may be that the Commissioners would point us to their statistical tables to discover an answer to our inquiries; but we should reply that these tables alone are inadequate for their solution; that among the archives the Commissioners possess are many papers needed to supplement their published tables, in order to convert them into trustworthy data whereon to base conclusions; that the Commissioners have means and opportunities for utilising their statistics, and also collateral information resulting from their wide experience and observation, which would enable them to arrive at more satisfactory and secure results than any private individual can do. Again, if the Commissioners hesitate to burden their ordinary official reports with these investigations, as matters somewhat out of place they certainly might, from time to time, publish a supplementary or special report devoted to such, as they did some years since as to the result of their inquiry respecting the condition of the insane in workhouses. Or if they feel qualmish about increasing the public printing bill, they might as individuals find fitting vehicles in the public press for conveying their views and inferences.

The sum and substance, therefore, of the foregoing remarks comes to this, that there are many important and some pressing questions concerning the lunacy of our country which want

elucidation and solution, and that no more fitting instrument to the end appears than what is furnished by the Lunacy Office.

We will now turn our attention to what information respecting the insane the reports before us do contain. Among other subjects possessing a wide interest are: the numbers of the insane; the comparative mortality of the last years; the notice of the various accidents, of their recoveries, and of the various other facts, of these general facts

moment to others than those who are superintendents of asylums, who are necessarily mainly interested in the representations published by the Lunacy Commissioners regarding the state of their establishments, their excellences or defects, their management, and the condition and behaviour of their inmates and their employés.

These revelations to the public of the internal economy of asylums are, we presume, regarded as made in the interests of the insane, and calculated to encourage the objects of official commendation in their good ways, and to inflict chastisement upon the unfortunate superintendents who fall under rebuke; but, in some instances, they must cause simply annoyance, and especially where the matter officially found fault with, is one of opinion, or mainly so, on which the visitors think differently from the asylum manager. If an individual would carefully read through these entries made by the Commissioners of the results of their inspections, and note the chief of them, we can imagine he might derive some information with regard to the views, both of those gentlemen and of the medical officers of asylums, respecting the moral and medical treatment, and the general arrangements for the discipline and comfort of patients in vogue at the present day. He might further get a fair insight into the character of the accidents and suicides which occur in asylums, and draw inferences as to their causes and possible modes of prevention. For ourselves we are indisposed to the task, and have only to remark that, from a cursory examination of the official entries, we are impressed with the conviction that, as well in public as in private asylums, the superintendents are animated by the utmost kindness towards their patients; that they do their best to make their enforced detention as agreeable as possible to them, by inventing and carrying out all sorts of diversions and amusements, and by encouraging useful employment, and that they do all this at much self-sacrifice and trouble.

One point more appears to call for observation, viz. that the employment of mechanical restraint has somewhat revived

in our asylums, and that an increasing opinion obtains that it is, in some instances, a necessary and a salutary means of treatment or management. This circumstance betokens a considerable revulsion of sentiment within the last ten or twelve years, or since the decease of the energetic advocate of non-restraint, Dr. Conolly, who by his personal influence and his teaching had elevated non-restraint almost to a principle of treatment, wellnigh universally accepted in Great Britain, and appealed to as a mark of greater progress in the humane treatment of the insane and as a system peculiarly English. This is not the place to discuss whether this change that has come over our psychiatrists is one marking decadence in opinion and practice, or whether it may be taken as a natural recoil from an exaggerated hypothesis and practice to a safer and truer equilibrium. But this much appears, that the Lunacy Commissioners have lapsed from their old ardent advocacy of non-restraint, for which they battled many years with recusant asylum physicians. But if mechanical coercion in the shape of restraint put upon the bodies of lunatics have somewhat revived in practice, there is in other directions, an extension of the practice of non-restraint. Much more out-door liberty beyond the asylum walls is allowed than formerly, and the necessity for walled or safely enclosed airing-courts, constructed in connexion with the several chief divisions of asylums, is no longer recognised, or, at most, not to the same extent. But a yet further extension has been brought about in some Scottish asylums, where locked doors to the rooms of patients have been dispensed with and liberty of egress and ingress permitted.

In their notes on the Fife and Kinross District Asylum the Scottish Commissioners write,—

“None of the airing-courts have been in use for nine months—all the patients take their exercise in the general grounds. This change is said to have proved beneficial in various directions, and the propriety of pulling down the walls of the airing-courts is under consideration. In visiting the male side of the asylum every door was found unlocked. On the female side, only three doors required to be opened by a key. Of the 248 patients in the asylum, 220 occupied unlocked rooms. This fact involves more than the mere removal or abatement of the sense of imprisonment. It has this most desirable result on all the patients, but to a very large and increasing number it means actual freedom to come and go at pleasure. It is admittedly of importance to avoid the mere appearance of restraint, but much more than this is done here, the freedom accorded to the patients being real as well as seeming. On the male side, in so far as locked doors were concerned, no difference was seen between the arrangements of this asylum and those of an hospital for the treatment of ordinary diseases. It is satisfactory

to be able to add that the effects in the management are to render it easier and cheaper."

We have referred in preceding remarks to a constant advance in the numbers of lunatics. The increase in England between the 1st of January, 1875, and the 1st of January, 1876, amounted to 1,123; and the total number, exclusive of 245 lunatics so found by inquisition, reached 64,916. In Scotland the total number on the 1st of January, 1875 (for, from some reason inconceivable to non-officials, the Scottish Commissioners lag a year behind the English in getting out their statistics), exclusive of unreported lunatics maintained in private dwellings was 8225; this presents an increase of 156 upon the return of the preceding year.

Of the sum total of English lunatics 57,407 were paupers, and 7509 private patients.

Of the total number in Scotland, 6700 were paupers; 1476 private patients, and forty-nine supported at the expense of the State as criminal lunatics. For the sake of comparing the last-named division with that in England we may note that there were 280 lunatics in the State criminal asylums, in the latter kingdom counted as private patients, and 228 enumerated as pauper criminals.

From the foregoing figures we find that the per-centage of pauper lunatics to the total number of insane of all sorts was in England 88.4 per cent. and in Scotland 81.4; whilst that of private patients amounted to 11.5 in the former, and to 17.9 per cent. in the latter division of the kingdom. In both countries the ratio of pauper to private lunatics advances. In Scotland, since the 1st of January 1858, the gross increase of lunatics on the registers has been 3253, but within that period 519 have vanished from the list of private patients, and 313 from private dwellings. The transference has, in fact, gone on from private establishments to public, so that the latter exhibit an increase of 2321 pauper lunatics on their books since the commencement of 1858.

The augmenting ratio of paupers in English County Asylums admits of partial interpretation by reason of the same common practice of removing patients from private to pauper asylums. There is an increase of 1606 pauper lunatics in county and borough asylums, and a decrease of inmates, to the number of 352, in licensed houses. The building of a new asylum for Kent and the enlargement of asylums in several other counties will account, as the Commissioners observe, for some so transferred, but the fact is more fully explained by this circumstance, viz.:

"That the pecuniary advantage to unions arising out of the Parliamentary allowance of four shillings per head per week for every

pauper patient maintained in an asylum has, in some counties, contributed to increase the number in these institutions, by the removal thereto of many patients who, but for such inducement, would have been retained in workhouses. It will be observed that the increasing ratio of the total number of pauper lunatics maintained in asylums has, during the last two years, been accompanied, as a rule, by a decreasing proportion kept in workhouses, and the per-centage of out-door paupers, who are boarded with their relatives or others, has continued to diminish." (p. 3.)

The general inference to be drawn from the two official reports is, therefore, that the increase of lunacy is pre-eminently among pauper lunatics; that parochial authorities, perceiving the pecuniary relief to local taxation to be obtained by transferring the insane occupants of their workhouses to public asylums and so throwing a considerable proportion of the cost of maintenance on the consolidated fund, avail themselves, without regard to the necessities of the cases so transferred, of the opportunity; and that a not inconsiderable number of the community are not above shifting the cost of the maintenance of their insane relatives upon the general rates by placing them as inmates of pauper asylums.

If these inferences be correct, and the facts and figures of the reports before us leave no doubt of this, we need not be astonished at the constant demands for new asylum buildings. But by yielding to those demands we are assuredly encouraging abuses. It is a well-known fact, that the opening of a new asylum has an attractive power and brings to light many lunatics not reckoned upon, particularly incurable ones, who usurp the accommodation prepared for others. This circumstance affords an indication of the readiness of people to rid themselves of their own proper burden at the public cost, and consequently an indication of an abuse. So, likewise, is the removal of chronic lunatics and imbeciles from workhouses to asylums constructed with special reference to the treatment of acute cases. And the great question opens up, how far is it desirable to segregate all insane folk in special public institutions; to remove them from home associations and influences, and to sever those bonds of sympathy which ought to exist between the afflicted and their relatives and act as a motive power for calling out the highest and best impulses? And it may be put as a collateral question, whether the transfer from home to an asylum is so generally an advantage to the insane as is supposed?

From the Report of the English Commissioners we gather this amount of consolation, that the total increase in the number of the insane was less last year than in any year since 1859; and

that this decrease was marked in twenty counties. Nevertheless, the ratio of persons of unsound mind to the population of the country continues to rise. In 1859 it stood to 10,000 of the population at large, at 18·67; in 1876, at 26·78. In Scotland, in 1858, the proportion was 19·1, and in 1875, 23·0. In both divisions of the kingdom the increase has been progressive, but in a higher ratio in England.

Again, although both in England and Scotland there has been a marked decrease of pauperism in the population at large, the proportion of pauper insane to paupers has gone on augmenting; so that in the former of the two countries it has advanced from 3·68 per cent. in 1869 to 7·62 in 1876; and in the latter, from 6·8 in 1868 to 9·7 in 1875; the English figures here again showing a more considerable rise.

Unhappily the prospects of brighter days with respect to the rate of accumulation of the insane are not encouraging; for with all the improved accommodation and moral treatment, of which boast is made of late years, there are no signs that treatment is more successful. There is no augmentation in the ratio of recoveries to admissions; this still lingers at the low rate of 34 per cent. In the "registered hospitals" it reaches the maximum of 38·56; these hospitals having the advantage of treating a larger proportion of recent cases. In the Scottish royal and district asylums this same rate is attained, and in the private asylums of Scotland the much more satisfactory ratio of 50 per cent. on the admissions.

On the other hand, death carries off among all classes of lunatics in England an average of 10·28 per cent.; the aggregate number of deaths being taken with the aggregate number resident. In the county and borough asylums and licensed houses the per-centage is 10·77, whereas in the Scottish royal and district asylums it is only 8·1. In both England and Scotland the deaths of single patients are at a lower rate; the returns from the former country showing 5·67, and for the latter 6·5 per cent. In dealing with these last figures, however, it must be noted that the class of private patients appearing in the English table are persons maintained at their own cost, whereas those included in the Scottish table are paupers boarded out in private dwellings.

Taking the single year 1875, and comparing its results as to recoveries and deaths with those of former years, the English Commissioners put it upon record:

"That the recoveries have been nearly 4 per cent. lower than those of the previous year, and 1·5 per cent. below the average of the last seventeen years. The mortality has also been somewhat unfavorable, having been about ·5 per cent. higher than the previous

year, and than the average mortality of the last seventeen years. The larger proportion of chronic cases included among the admissions of last year must be accepted as an explanation of the diminished ratio of the recoveries; whilst to the severe weather at the commencement of 1875, which carried off an unusual proportion of aged patients, must be attributed the higher rate of mortality shown by the figures of the year."

A moment's consideration of the ratios of recoveries and of deaths at once makes plain the cause of the notorious accumulation of chronic lunatics in asylums. It is well worthy of inquiry why only from 30 to 38 per cent. of those admitted into asylums are discharged recovered. For psychological writers assure us that insanity is a curable disease, and that by prompt and proper treatment 70 and even more per cent. of its victims may recover. One reason assuredly is to be found in the oft-reiterated complaint of the number of incurables taken to asylums. But how comes this to pass, or at least to go on year after year? We can well allow for it when a new asylum is opened, and all the workhouses in its district proceed at once to transfer their old patients to it, and the many unknown or little known poor lunatics scattered up and down the country are taken to it by their relatives, who gladly rid themselves of their burden and cost; but we do not comprehend why a perpetual stream of incurables should be forthcoming. The chronic cases must have once been acute, or at least have had a beginning when a chance of recovery was possible, and it may be asked why did they not gain admission to the asylum when in the recent and more hopeful condition? That they did not, and do not, is a proof there must be something wanting in the laws of lunacy or in the machinery for administering them. There is a clear demand for some plan to secure the immediate transfer of lunatics—at least of those who by their lunacy become chargeable—to an asylum where they can receive individual attention, and obtain medical as well as moral treatment for their malady; not to an institution simply because it is called an asylum, and where after admission they will be treated only as so many additional units in the throng of hundreds, to be simply subjected to the routine and discipline of those around them, and among whom they soon become almost lost to observation;—for if so their prospects of cure may, at the best, be little improved by their admission.

Were we to go fully into this question we should have to embark on the wider one touching the best forms of accommodation to be provided for recent and for chronic cases, but, here at present, we can only express the conviction that the present organisation of our asylums is not calculated to keep under the accumulation of the insane by promoting their recovery.

It is but just to the English Commissioners to note that they fully perceive the importance of early treatment, and regret the practice of keeping recent cases in workhouse wards. This practice does not obtain in Scotland, for the reason that the Scottish Commissioners have control over the poor-houses, and no insane patient can be placed in them without their cognisance and sanction. It would be well for their English colleagues to acquire similar control by Act of Parliament.

Nothing can display more vividly the condition into which English asylums have fallen than the following paragraph from the report :

"The proportion of patients in the county and borough asylums who on the 1st of January last were deemed by the various medical superintendents to be *curable*, amounted to only 7·17 per cent. of the total number, as against 7·47 per cent. on the 1st of January, 1875." (p. 23.)

We are glad to find the attention of the English Commissioners has been called to the condition of pauper lunatics boarded out with their relatives or with strangers. We consider that the machinery provided by law for their supervision is very defective, and that that obtaining in Scotland is far superior. In England they have almost universally escaped the notice of the Commissioners ; in Scotland they are under the direct supervision of the Lunacy Board, and particularly under that of two Assistant Commissioners.

It would seem that representations reached the English Commissioners of the neglected and wretched state of these single patients boarded out in several Welsh counties—in which, by the way, their proportion far surpasses that in English counties. In consequence of the statements, two members of the Board personally hunted up these solitary lunatics, and it is very gratifying to learn from them that the state of things was not nearly so bad as was represented.

"At the same time," they report, "it is only by comparison with the very miserable cottages and beds of the ordinary cottagers that some of the cases can be considered fit to remain where they now are, and it was only in consideration of the kindness shown to these poor people by their neighbours, if living alone, and by their relatives, when residing with their families, and, above all, to the insuperable objection they felt to being sent to the workhouse, that we now refrain from making any suggestion for such a course by recommendations to the Guardians. As regards removal to the asylum, we only found two cases for it, and in those instances our orders were made solely with reference to the mental condition of the patients, their accommodation being in both cases satisfactory."

In conclusion, they report that the allowances made for the

maintenance of these poor people is frequently too small, and recommend that more care should be bestowed on diet, cleanliness, and the bedding.

This result of this one special inquiry conducted by the English Board harmonises on the whole with the recorded observations of the Scottish Board which has ever since its institution given great attention to single pauper patients boarded out in cottages, and has rather fostered the system, in the belief that it presents advantages in the case of many chronic cases over detention in asylums.

Before dismissing the Scottish Report, and concluding this paper, we would take occasion to notice one topic on which the Commissioners have instituted special statistical investigation: it is that of the prevalence of general paralysis. "During the ten years 1865-74, 524 deaths from general paralysis were reported. Of these 90 referred to private patients and 434 to paupers," and taking both classes of patients together the ratio of deaths was 1·6 per 100,000 of the population, one fifth being private and four fifths pauper.

On classifying 434 pauper patients according to the parishes to which they were chargeable, the localities, called town-districts by the Registrar General, yielded an annual death rate of 1·9 per 100,000 of the population; whilst urban and insular localities yielded only 0·8, or less than one half the former rate. Further, when density of population was taken into account, the divergence between town and country became greater; the death rate reaching 2·1 in the principal towns; 1·3 in the large towns, and only 0·7 in small towns and rural districts. These figures bear out the current belief that general paralysis is especially a disease of cities. The same rule also holds good of nervous diseases. Thus, for every 100 deaths in the country from diseases of the brain and nervous system, there are 170 in towns; and for every 100 arising from delirium tremens in the rural districts there were 217 in towns. But, by comparison, the increase of the death rate, consequent on general paralysis, and in relation to density of population, is greater than the same from other nervous maladies.

Other tables subjoined in the Report confirm the opinion generally held of the great exemption of the female sex from general paralysis, of its being a disease especially of middle life, and of its proving fatal in a very large proportion of cases (75 per cent.) within two years of its recognition.

Further analysis of these official documents want of space compels us to forego.

VIII.—Recent Studies in Nerve Histology.¹

THE following dedication informs us to whose rare liberality and public spirit the scientific world is indebted for the magnificent publication before us.

“To the highborn Lord Carl Baltzar Ernst, Count Von Platen, by whose noble aid they were enabled to publish the result of their observations in a manner worthy of the subject, the authors respectfully and thankfully dedicate this work.”

This single volume before us would, of itself, be a lasting monument to the liberality of this nobleman, but it is only the first half of a work, which when completed, will stand as a model of deep and exhaustive research.

The portion of the work as yet published consists of a folio volume of 220 pages of text, accompanied by 39 interleaved lithograph plates of the same size, both coloured and plain. From the preface we find that the investigations here recorded, were commenced in the year 1869.

A desire to clear up the question of the connection between the under surface of the arachnoid membrane and the upper surface of the serous investment of the brain and the spinal cord, afforded the prime motive for these investigations. Commencing with an examination of the anatomy of the subarachnoid space, it was found by the investigators that the further they went the further they were tempted, nay compelled, to go, one point leading to another; one conclusion giving rise to another question, with an ever-widening circle of investigation before them.

Thus, for example, the authors found on injection of the subarachnoid cavity that unsuspected communications, and therefore previously unexplored fields of research were, one after another, brought into view.

To give one instance of a point satisfactorily solved, and which arose fortuitously in the course of their investigations, we may mention the demonstrations of the existence of the connection between the general lymphatic system and the serous cavities of the higher organs of sense and of the cerebral serous spaces themselves. The description of the subarachnoid space necessitated a description of another not so well known cavity; we refer to that existing between the dura mater and the arachnoid. Examination of this latter, besides

¹ *Studien in der Anatomie des Nervensystems, und des Bindegewebes.* Von AXEL KEY and GUSTAV RETZIUS. Erste Hälfte, Stockholm, 1875.

Studies in the Anatomy of the Nervous Centres and their Connective Tissues. By Professor AXEL KEY, and GUSTAV RETZIUS.

revealing the connection between it and the serous cavities of the higher organs of sense, leading to the discovery of the existence of lymphatic spaces in the peripheral nervous system.

This discovery was followed by a more exact examination of the structure and composition of that system, and finally, by a critical inquiry into the structure of the various forms of connective tissue. The membranes of the brain and spinal cord afforded prototypes of the different varieties of this tissue. For the sake of distinction these varieties have received the following names, referring mainly to their special characteristics: "loose connective," "connective, rich in lymph spaces," "fibrous," "firm," and finally, that found in the Pacinian corpuscles, which has received the name of "lamellar connective."

Thus, as before stated, one step led to another and, like the stone thrown into the pond, the primal discharge of investigating energy spread in ever widening circles.

Turning now to an examination of the heads of the chapters, we find that pages 1 to 57, are devoted to a historical review of the known facts concerning the membranes and serous spaces of the brain and spinal cord, and that the remaining 163 pages are devoted to the following subjects:—

The sub-dura-mater space (*Subduralraum*) of the brain and cord; the subarachnoid space; and the general structure of the softer membranes of the brain and cord; the open communications between the ventricle and the subarachnoid space; the minute structure of the membranes of the brain and cord; the general and special histology of the softer membranes; the histology of the dura-mater; the arachnoidal tufts, or so-called paccionian glands; the coverings, and the spaces in the coverings of the optic nerve, and their connection with the coverings and serous spaces of the brain; the special and general histology of the optic nerve; the communications between the coverings of the auditory nerve and the labyrinthine cavities of the temporal bone, and the connection between the cerebral serous spaces and the lymphatics of the nasal mucous membrane.

At this point it seems fitting to indicate what, in our view, is a standing objection to the works of nearly all foreign authors: we refer to the habit of devoting so large a space to what they call historical records. In these are to be found not merely abstracts giving the views of the various authors referred to, but also extended quotations.

The work before us is no exception to this rule. As we before stated, 57 pages are devoted to these historical records, and besides that space, each division of the subject is preceded by an historical retrospect. The attempt to follow our authors in this field would lay us open to a similar charge. We shall, therefore, simply remark

that of the writers whose works are quoted, by far the majority are German.

Passing then over the first 57 pages of the text, we come to the chapter which has the following title, the 'Subduralraum,' of the brain and cord; by the word 'Subduralraum,' is meant the space between the dura mater and the arachnoid membrane.

Among the authors referred to on this subject, are the following: *Galen, Vesalius, Vieussens, Bichat, Ecker, Majendie, Luschka, Schwalle, Recklingshausen, Arnold.*

The space in question, between the dura mater and the parietal layer of the arachnoid, is demonstrated by MM. Axel Key and Retzius to be continuous over the whole cerebral, medullary, and spinal cord surfaces. It is prevented from being a perfectly uninterrupted cavity by the passage through it of blood-vessels and nerves, and by fine thread-like attachments to the subjacent arachnoid. It is entirely disconnected from the arachnoid cavity, there being no direct communication whatever. This is proved by the fact that when the subdura-mater space is carefully injected with coloured fluid, no trace of the fluid is to be found in the arachnoid cavity. It must not, however, be supposed that the cavity between the dura mater and the arachnoid is a closed sac, without communication with any other part of the body. The contrary is the case. By means of the nerves and blood-vessels which pass through it, it is placed in communication with the general lymphatic system, and by means of the canal of Bichat, through which the venæ galeni pass under the splenium of the corpus callosum, it is in direct communication with the third ventricle, and therefore with the other ventricles, and likewise, in an indirect manner, with the arachnoid cavity itself.

By means of injection, communications have likewise been shown to exist between the "subduralraum" and the serous cavities of the higher sense organs.

The points in the anatomy of this region are well shown in the plates, of which we would here say that they are surpassingly admirable, and alone would render the book of great value.

The next chapter treats of the "subarachnoid space and the general anatomy of the pia mater of the brain and cord."

The subarachnoid space is described as existing between the arachnoid fold which is attached to the dura mater and the fold attached to the pia mater and surface of the brain and cord. In the examination of this space our authors have used various means, but principally injections, fluids in differing consistence being employed. In some instances the body has been thoroughly frozen, whereby the investigation has been rendered more easy and of greater accuracy. The result of the different processes pursued is a complete description of this space, commencing with its spinal portion. This last is divided

into two spaces, separated from each other by the ligamentum dentatum, which is described as forming a kind of diaphragm.

Among other structures contained in these spaces, of which the minute anatomy is described, we have the ligamentum dentatum and its relations to the cord, the pia mater, the arachnoid, and the spinal nerves. Accurate measurements have also been taken of the different parts, and of the subarachnoid space in the spinal canal. These, however, were found to vary so much in different individuals, that only relative conclusions could be deduced from them.

Ascending from the region of the cord to that of the medulla oblongata, the arachnoid space was found to expand in all directions. Posteriorly the injection was seen to spread over the surface of the cerebellum for a considerable distance into the fourth ventricle, through the opening which exists in the arachnoid membrane, which forms the posterior inferior wall of the ventricle. The space thus filled by the injection has been named by our authors *cisterna magna cerebello-medullaris*. Its extent and connections are admirably portrayed in the plates referred to in the text. Anteriorly the injected fluid passed at the region of the pons into three spaces, one central and two lateral, which have been called the *cisternæ pontis media et laterales*. The formation and structure of each of these spaces is given, the description again being greatly aided by the plates. In the middle of the central space runs the basilar artery. The formation of this central space is shown to be of some importance with reference to its influence on an aneurism of the artery. Through the lateral spaces in this region pass the nerves which take their origin at this part.

In passing forwards and upwards the injection found its way into two important cavities, named, from their position, *cisternæ intercruales, superficialis et profunda*. These two spaces, though, of course, freely communicating, are partially divided by the basilar artery at its bifurcation.

In front of the pedicle of the pituitary body is another space, which has from its position received the name of *cisterna chiasmatis*. Another median space is, on the same principle, called the *cisterna lamina cinereæ terminalis*. This is situated above the chiasma of the optic nerves, and is prolonged to the anterior genu of the *corpus callosum*, being also continuous with a subarachnoid space which passes along the upper surface of that body.

Spreading out laterally from the pons Varolii there are two subarachnoid spaces called *cisternæ fossæ Sylvii*, and, lastly, another space, intervening between the cerebellum and cerebrum, starting from the pons Varolii, is named the *cisterna ambiens*.

The connections of the arachnoid cavities and the ventricles, and the structures found in these various cavities are all critically examined.

The chapter concludes with observations on the Pacchionian glands, which are considered to be tufts of the arachnoid membrane, serving as media for the passage of the cerebro-spinal fluid into the venous system.

The next chapter, which treats of the open communications which exist in the lower wall of the fourth ventricle with the sub-arachnoid space, is, perhaps, one of the most interesting. The historical retrospect serves to show that the subject is one upon which, although touched by many authors, much light had not, up to the present time, been thrown. From the description now given it seems as though the whole matter has been cleared up, and that nothing whatever is left for future searchers in this region to discover.

The posterior inferior wall of the fourth ventricle is formed by an expansion of the arachnoid from the medulla, stretching from the level of the point of the calamus scriptorius to the cerebellum. An accurate anatomical description of this structure is given. It is represented to us as being, to translate the words literally, a tegumentary residuum of the primordial ventricle wall. The epithelial connection with the floor of the ventricle is clearly described, and the ependymal covering of the choroid plexus (plexus choroidea media) is by means of the figures admirably demonstrated.

The main subject of the chapter, however, as shown in the title, is not so much this "tegumentary residuum" as the openings which exist in it, and which afford free passage to the circulating cerebro-spinal fluid. These openings are three in number. One which has been noticed by Majendie, and, therefore, named after him, is here described as a round, or oval, or obtuse angled polygonal space in the membrane. The difficulty of demonstrating this opening is very great, owing to the fact that when the brain is removed from the skull in the ordinary way the membrane, which is very slender, is generally torn; the authors, therefore, give directions as to the best method of removing the brain for the purpose of seeing this structure. With regard to the different borders of this opening we are told as follows:

"The borders of this opening form, on either side, the thin lower wall of the fourth ventricle, the *tela choroidea inferior*. From the lateral borders proceeds, on either hand, a fine fringe to the point of the *calamus* (the obex), by means of which the opening is maintained in its integrity, and at the same time allows of the exit of the *plexus choroidea* from the medulla at some distance from the obex, and the separation of the borders of the opening from the *fasciculi graciles*. The superior posterior edge of the opening is also formed by the *tela choroidea*, but its arrangement is somewhat peculiar. From this point it takes a tongue-shaped form, tapering and flattening from the opening to the under surface

of the vermis, lying on the floor of the valleeula . . . From this tongue-like prolongation of the *tela choroidea* starts on either side, the commencing choroid plexus of the fourth ventricle."

The two other openings through this expansion of the arachnoid are described as being on either side of the medulla, between it and the cerebellum, and are to some degree hidden by the roots of the glosso-pharyngeal nerves. The openings are larger than the first-named one; they also allow of the passage of a choroid plexus, which belongs peculiarly to the lateral expansions of the fourth ventricle into the cerebellum. The great peculiarity of these openings, however, is the fact of the valvular action attributed to them by the authors.

The process is shortly as follows:—With every inspiratory effort the cerebro-spinal fluid is forced through the foramen of Majendie, and so on through the ventricles. When, however, the fourth ventricle becomes full, and the lateral expansions dilated, the lateral apertures are drawn open, and, with the expiratory movement of the chest, allow of the escape of the distending fluid. The following names are proposed for these openings, viz. *aperturæ laterales ventriculi quarti*; and for the foramen of Majendie, *apertura inferior ventriculi quarti*.

The chapter finishes with an account of the pathological conditions affecting these structures, and with a short notice of the recorded abnormalities in their form.

The succeeding chapter is devoted to a description of the soft membrane ("der weichen Haut") of the brain and cord. The parts included by the authors in this expression are the layers of tissue between the inner surface of the dura mater and the nervous substance of the brain or cord. These are divided into an outer and inner and an intermediate layer. The outer layer is called the arachnoid, the inner the pia mater, and the intermediate the sub-arachnoid tissue.

The histological description is preceded by a short sketch of the various processes employed in preparing the specimens examined, and followed by a careful and apparently accurate description of the minute anatomy of the structures met with, accompanied by numerous illustrations.

The so often mentioned fibres which stretch between the different layers (called here "Balken," *trabeculae*) furnish, in their minute structure, abundant scope for the painstaking and persevering researches of the investigators.

These structures are found to consist of bundles of spiral fibres covered with a thin, brittle, transparent sheath. These, by careful manipulation, are divisible into finer fibres, which are found to be united to each other by still finer connective tissue. Each original fibre

is not completely isolated from its fellow, but admits of dichotomous or even more various division, and of similarly varied connections with others. A peculiar transverse marking and varicose appearance of these fibres, when isolated and examined under certain favorable conditions, is found to be due to transversely arranged fibrillæ.

The sheaths of these bundles afford well-marked specimens of the nucleated bio- or proto-plastic masses, and the bundles themselves of white and yellow fibres.

The membranous portions of the structures treated of in these chapters are also shown to be essentially fibrous in structure, and are considered as networks, close, indeed, but still simply networks formed by the outspread terminations of the trabeculæ. Blood-vessels and nerves permeate these membranes, indeed, there exist here and there trabeculæ which contain nerve fibres.

The following chapters enter with still more detail into the histological appearances of these membranes, and the description becomes more topographical.

The minute peculiarities presented in different regions are pointed out with extreme care and precision, the description of them being given in such a manner as almost to preclude the possibility of mistakes arising from the use of words to which more than one meaning can be applied, as is so often the case with descriptions of what is seen under the microscope.

The dura mater is next subjected to a similar examination, and the result is equally complete and satisfactory; the use of injections rendering the microscopical discoveries here recorded of great value. The connections existing, as pointed out in the notice of the chapter on the subduralraum, between this part and the general lymphatic system, and the serous cavities of the higher organs of sense, &c., are here not only proved, but their structure is also critically examined. In connection with this part of the subject, one point strikes us as particularly interesting. It is, that the so-called lymphatic perivascular spaces, as originally described by His, are clearly proved-not to exist in the healthy brain. The injection of the subarachnoid space shows that there is no communication between it and the spaces which are sometimes observed around the blood-vessels. The pia mater is described as forming a boundary in the shape of a funnel around each blood-vessel as it enters the brain. Beyond this the injection does not penetrate. The pia mater is firmly attached to the vessel, and accompanies it, as *tunica adventitia*, but not as forming a lymphatic tube or anything of the sort.

Similar views are held by some English pathologists, who have as the result of their investigations, maintained that these perivascular canals do not normally exist as lymphatic spaces, but are the result of morbid action.

The next chapter has for its subject the consideration of the Pacchionian granulations (glands?), or, as the authors prefer to call them, the arachnoid tufts. Up to the time of the publication of the present work, very little seems to have been known of these structures. There seem to have been some very indefinite ideas as to their nature, doubts as to whether they were not morbid products or whether they had any connection with the arachnoid, whilst their existence in early life has been emphatically denied. Nothing whatever had been advanced which throws light upon their function. So far at least we may conclude from an examination of textbooks and a perusal of the historical retrospect here given.

With a view of setting at rest these vexed questions, the authors have described new methods for the examination of these structures and the results they have thereby attained, and conclude with satisfactory deductions concerning the physiological functions of these peculiar formations. An outline of these results and deductions is as follows. In the first place these structures are not morbid productions, but are present at all stages of life. With regard to their minute structure, we are informed that these granulations or glands are in reality tufts of arachnoid tissue protruding through the dura mater in order to reach the various sinuses which abound in that membrane. In structure they consist of balloon-like prolongations of the arachnoid, and internally are composed of trabeculæ stretched in all directions, and freely communicating with each other, leaving, however, numerous differently sized spaces in their meshes. At the periphery of these tufts the trabeculæ seem to spread out by their roots so far as to interlace and to form a continuous membrane covered on the outside with a fine epithelial layer. On the outside of this layer is a space, also traversed by trabeculæ, though these are much fewer in number. At the end of those farthest from the tufts they run into and coalesce with a thin layer of dura mater, or of membrane continuous with the dura mater. The space between this and the tufts is called their subdura-mater space; it is continuous round the pedicle of the tuft, or at the point where it penetrates the dura mater with the corresponding cerebral space. By injecting the subarachnoid space in the spinal cord, using in the process steady and firm pressure on the fluid, these tufts have been found capable of injection, the fluid entering within their meshes as in a sponge. The employment of rather more force causes an exudation of the injected fluid into the sub-dura-mater space of the tufts, and on the complete injection of this space the fluid oozes through into the venous tissues and mingles with the blood there. Further, careful injection of the space underlying the dura mater, has shown that the fluid will go the reverse way; that is, it will penetrate the tufts and cause their injection and erection, as when the subarachnoid cavity itself is injected. These points being set

at rest, experiments were made to test the amount of the pressure exerted first by the blood in the sinus; 2nd, by the cerebro-spinal fluid in the sub-arachnoid and dura mater cavities. The results, which are tabulated, show that both in inspiration and expiration, the pressure of the cerebro-spinal fluid is the greater. Taking this fact in view, and bearing in mind that the cerebro-spinal fluid is of less specific gravity than the venous blood, the inference is, that the process of osmosis is perfect in this wondrous mechanism, whereby the cerebro-spinal fluid is made to mingle with the venous blood of the various sinuses of the dura mater.

Turning next to the examination of the sheaths and spaces therein of the optic nerve, our authors demonstrate clearly that these are continuous, and in direct communication with the same structures on the surface of the brain and cord. The dura mater terminates in the sclerotic, the arachnoid on the internal surface of the dura mater in the cul-de-sac formed by the sub-dura-mater and subarachnoid spaces at the back of the eyeball. The pia mater also terminates at this point.

By means of injections these spaces have also been found to be in communication with the larger cavities of the brain and cord.

Minute examination of the optic nerve itself has revealed the existence of trabeculæ of the pia mater winding in a net-like manner among the nerve fibres. With these trabeculæ run the blood-vessels. On the inner surface of the meshes thus formed are found between the trabeculæ and the nerve fibres a structure, or rather a series of structures, which deserve attention. They are minute cellular bodies with thread-like processes, which in the figures here given afford a strong resemblance to those given by Deiters of the so-called spider cells described by him as existing in the neuroglia of the nerve centres.

There next follows a minute account of the structure and the connections of the nerve in the lamina cribrosa. The spaces which exist in the papilla optici, the lymph spaces of the nerve itself, their connections with the serous space between the layers of the eyeball, and with the subarachnoid and subdura mater spaces in the brain, are all demonstrated, partly by means of injections and partly by means of the microscope.

Finally, we have two short chapters devoted to showing the connections between the serous labyrinthine space in the temporal bone and the cerebral spaces. The communication of these latter spaces with the lymphatics in the mucous membrane of the nostrils is demonstrated, as in other cases, by means of injection.

The above notice of this book is enough to show its extreme value. As we before stated, it stands as a monument, not only of the scientific attainments of the authors, but also of the generous liberality of the Count von Platen. Surely such an example is

well worthy of being followed ; and on this account we recommend it, with all due respect, to those noblemen who, from the largeness of their minds and the liberality of their actions, form one of the chief ornaments of English society. Surely, if it is possible for Sweden, which certainly does not reckon as one of the wealthiest States in Europe, to produce such an instance of private generosity and enlightened intelligence, it must be possible for England to do not merely the like, but still better. Let us at least hope so, and meanwhile ascribe the full credit of such a noble act where it is properly due.

IX.—Hamilton on Fractures and Dislocations.¹

THE treatment of these most common and important accidents has, all through the history of surgery, been more than any other department unfortunately surrounded by a sort of halo of romance or cloud of mystery. Even in this nineteenth century, and in circles where education might be supposed to have eradicated superstition, we find an idea lingers that the *bone-setter* is a natural genius above and beyond the fetters of circumstances, that by a sort of heaven-sent power he can perform his cures without any previous study of the anatomy of the part, that by his dexterity or strength he can replace bones that the regular practitioner in his ignorance believes not to exist, and restore joints to their normal relations which the hospital surgeon may have described as already reduced.

The more ignorant, rude, even brutal, the genius may be, the more do his votaries admire his skill ; the unexpectedness, the unlikelihood of an uneducated peasant knowing more than a hospital surgeon, who may have spent years in study and a life in practice, add to the interest he inspires and the marvel of his cures. He has the great advantage also of having no misgivings ; he may lie just as loudly and as long as he likes, unrestrained by shame, uncontrolled by fear. If he is told that the “small bone” he has put in does not exist, he need not verify his diagnosis by dissection or demonstration ; he may simply lie the louder. But his great pull over the regular professional man is found in this, that only his cures are heard of.

¹ *A Treatise on Fractures and Dislocations.* By F. H. HAMILTON. New York, 5th edition. London, 1876.

The patient who was not ill, but who thought he was, goes to the quack and recovers, not because of, but in spite of, his absurdities or his incantations, brags to all his friends of the success, perhaps of the cheapness, of his wanderings; the patient who really was ill, and who is made worse, being thoroughly ashamed of his folly, never tells of his visit to the quack, and, therefore, can neither point a moral nor adorn a tale.

But we must honestly own, to the shame of our profession and to the discredit of the licensing boards, that some of the success of the quack is due to the inefficiency and ignorance of many members of the profession as to the diagnosis and treatment both of fractures and dislocations. Diagnosis is often difficult, sometimes at the time *impossible*. The surgeon is called to see an elbow some hours after a severe bruise or fall. The pain, swelling, and deformity are so great that it is scarcely his duty to give a diagnosis at all. He may commit himself, and say no bone is broken; he may not again see the case till it is obvious to patient as well as himself that he was wrong.

Again, he may diagnose correctly that a femur is broken; he may put it up in splints, according to the rules of the books, but he may have not noticed or not inquired as to the condition of the mattress on which the patient lies—may go day after day and see the splints still firm and patient uncomplaining, and yet find at the end of five weeks that the patient's thighs have sunk into a hollow in the bed, and that his limb is united at an angle. Such cases are frequent.

If every medical student would make a diligent and thorough use of his opportunities in the accident ward or out-door waiting room of his hospital, he would soon obtain a practical knowledge of the general principles of treatment of fractures which would render mistakes such as the above impossible; and if on going into practice he would try and master a careful systematic treatise like this one of Hamilton's, he would be ready to give a good opinion on and successfully treat almost any fracture or dislocation, however rare.

Dr. Hamilton's great work has achieved, and most deservedly, an almost unexampled popularity in this country as well as on the other side of the Atlantic. Already in its fifth edition, it may be said to have passed the stage at which criticism can either make or mar its success, and to have attained the calm atmosphere of a classical work. It may be interesting as well as instructive, in the short notice we mean to give of it in these pages, to pick out here and there points where differences in opinion imply and encourage varieties in practice, and as we proceed to compare now and again the teachings of the present with those of the past. The best work, as a standard of comparison,

is Sir Astley Cooper's great 'Treatise on Dislocation and Fractures of the Joints,' the eighth edition of which is now before us, published in London in 1832.

What have these forty-four years of progress done for the science and art of surgery in the domain of fracture and dislocation? In accuracy of description and in clinical insight we can hardly expect to find much advance upon Sir Astley. His description of the varieties of dislocations in their pathology and symptoms can hardly be improved upon; and though the last half century has given us many varieties of nomenclature, more or less refined and pedantic, practical surgeons do not invariably or even frequently adopt them. We still hear of dislocations of the shoulder—forwards, downwards, and backwards—and Sir Astley's descriptions of them are not less lifelike and instructive than those of the present day, where the same injuries appear under their new names of subglenoid, subcoracoid and subspinous.

In descriptions of treatment, however, and especially in cataloguing various instruments, such as splints, pulleys, racks, adjusters, &c., where Sir Astley Cooper has one device, the American surgeon has fifty; many useless, some dangerous, yet in the list many of great value, which will help a surgeon out of a difficulty, and only render him a little sad in thinking,—"Surely, there can be nothing in this line left for me to invent."

Choosing at random subjects for comparison, we find under the head of "Fracture of the Clavicle" that Hamilton gives a most elaborate and exhaustive summary of the opinions of surgeons as to its prognosis and treatment—that opinions and methods are alike contradictory and unsatisfactory. His own summary is in the following words:

"Finally, while I deprecate incautious assumptions in regard to the capabilities of any form of dressing for broken collar bones, a disposition to which is manifested by more than one advocate for special plans, I am ready to declare my preference for an apparatus consisting essentially of a sling, axillary pad, and bandages, to secure the arm to the chest. Among the considerable variety of dressings which I have used, this has seemed to me the most simple in its construction, the most comfortable to the patient, the least liable to derangement (if I except Velpeau's dextrin bandage and certain other forms of immovable dressings), and as capable as any other of answering the several indications proposed, while the patient is permitted to walk about" (p. 205).

If a *perfect* result is wished, and the patient is willing and able to lie on his back for three or four weeks, probably the

only certain method, and certainly the easiest, is to keep him flat on his back with a good long pad under his spine, so as to let the shoulders fall well back.

Passing to the frequent, and hence important, dislocation of the shoulder, though the nomenclature has been changed and somewhat complicated, Sir Astley Cooper's descriptions have not been improved upon. Hamilton retains the old names, but slightly alters the exact description of the suggested position of the head of the bone. The introduction of anæsthetics, which has made treatment more successful in old dislocations, has also greatly added to the list of accidents during and after reduction. It was not so easy to rupture an artery or pull off an arm while the patient was conscious and struggling as it now is when he is unconscious; and probably now cases of longer standing, and consequently of greater danger, are attempted by surgeons.

Fractures of the condyles of the humerus into the elbow-joint are so frequent, and so apt to be followed by troublesome consequences, that they are often described. It is curious to notice the different views taken as to the exact pathology and relative frequency. Sir A. Cooper describes most accurately, and had frequently seen the oblique fracture separating the *internal* condyle, of which Malgaigne had never seen a living example. Hamilton had seen several cases, but had much more frequently seen the oblique fracture separating the external condyle; the writer of this review in a pretty extensive hospital experience has seen at least three of the *internal* condyles for every one of the *external*, and believes both to be common accidents of youth and childhood. Sir Astley Cooper's treatment of rest, with the limb at a right angle and in a sling, cooling applications and early passive motion, is the one still most approved in this country; the splints recommended by Hamilton being much more likely to do harm than good.

Colles' fracture is another well-trodden arena of strife. A fracture exceedingly common, deforming, apt to be followed by bad results to the usefulness of the hand; the variety of splints is enormous, and their number is constantly increasing. Sir Astley recommends splints anterior and posterior, and careful bandaging, but ends his description with the discouraging remark that six months will sometimes elapse before motion of the finger is completely restored. Hamilton gives numberless plans more or less complicated, including one of his own, and he seems just on the point of discovering what will certainly be universally allowed in a few years, *i. e.* that much of our want of success in the treatment of this fracture is due to overdoing treatment. We have often put the question, Did

you ever see a case of non-union in a Colles' fracture? and have never received an affirmative answer; but you have often seen great stiffness both of the joint and fingers? Yes, that is the rule in the aged for months. Then why keep the limb so long in splints? and why trammel the fingers? For years the writer has never fixed the fingers in a Colles' fracture in the old or adult, and is every year shortening the period of splints at all. Hamilton has hit the chief point when he says (p. 304):

"In most cases all of our success will depend upon whether we employ sufficient force in the early stage of the accident and in the right direction. When once reduced it is easily kept in place."

The treatment of this lesion in the future will be early and complete reduction; short, and cautious use of splints, neither of which should ever compress or limit the movements of the fingers; and free and early passive motion if there is any tendency to stiffness from chronic or subacute inflammation in the sheath of the tendons. No historian of the surgery of fractures can omit from his record an account of the important services rendered by Dr. Gordon, of Belfast, in explaining the pathology and simplifying the treatment of Colles' fracture, as well as of many other fractures.

Passing in our rapid review to the fractures and dislocations of the lower limb, we would compare the account given by Sir Astley Cooper of the dislocations at the hip-joint with that of the later classic. In doing so, we almost marvel at the accuracy of the older surgeon as to the position and relations of the displacements; little has been learnt in that respect since his day. Hamilton incorporates, in his description of these dislocations in the present edition, the views lately so ably advocated by Dr. Bigelow, of Boston, as to the importance of the relation of the ilio-femoral ligament to the head of the bone. Hamilton, agreeing with Sir Astley that of the four chief dislocations of the hip, the one into the ischiatic notch is the most difficult to detect, calls attention to a most important diagnostic mark to which Mr. Syme has called attention, but which in England has not met with the notice it deserves; this is

"The arched form of the lumbar part of the spine which cannot be straightened so long as the thigh is straight, or on a line with the patient's trunk. When the limb is raised or bent upwards on the pelvis the back rests flat upon the bed; but so soon as the limb is allowed to descend the back becomes arched as before."

Hamilton also advises that, in difficult cases, an examination may be made by the rectum or vagina.

The treatment of hip dislocations has greatly altered since

Sir Astley's day ; the pulleys, adjusters, and other complicated apparatus being replaced in the great majority of cases by manipulation, aided by the thorough anæsthetised condition of the patient under ether or chloroform.

On the extremely important subject of the prognosis and treatment of fractures of the thigh, Hamilton is, and deservedly so, an authority of great weight. To him, far more than to any living man, is due the honour of having really put on a scientific basis the vague generalities which had been accepted by the profession and the public regarding what *ought to be* the result in cases of fracture of the thigh. Partly due to ignorance and carelessness, partly to the arrogant boastings of quacks both inside the profession and out of it, an idea had arisen that when a thigh was broken it was the surgeon's duty to mend it without shortening, that any loss of length in the broken limb was explicable only by the surgeon's neglect or maladroitness.

Hence actions for malpractice, conflicting evidence, much heart-burning and disappointment, and great discredit to the profession.

To Hamilton chiefly is due the credit of pointing out, both from his own careful measurements and examples, and also by quotations from the great masters of surgery, who had honestly given their own experience, that in the vast majority of fractures of the shaft of the thigh in adults there will be, spite of all care and all skill, a certain amount of shortening—not much, but still half an inch to an inch.

Cases there are, it is true, in which with due care absolute accuracy is obtained easily and certainly ; but these are subperiosteal transverse fractures, the result of direct violence ; with little displacement. These cases do well under any treatment or none ; and in the fat limbs of children, if once put in position, heal with astonishing rapidity, even with no splint applied. These cases encourage the do-nothing treatment of the late Professor Dudley, of Lexington, who at one time maintained that fractures were best treated by being put in position and bandaged with a common roller.

Any surgeon who has had to treat many fractures of the thigh from falls and other indirect violence, in which the oblique direction of the fracture favours the overlapping displacement which is begun and maintained by powerful muscles, will know how hopeless it is to expect absolute preservation of the length of the shaft, and agree with Hamilton in his excellently worded and carefully expressed doctrine, which we here quote :

“ First. That in the case of an oblique fracture of the femur

occurring in an adult whose muscles are not paralysed, but which offer the ordinary resistance to extension and counter-extension, and where the ends of the broken bone have once been completely displaced, no means have yet been devised by which an overlapping and consequent shortening of the bone can generally be prevented.

"Second. That in a similar fracture occurring in children, or in persons under fifteen or eighteen years of age, the bone may quite often be made to unite with so little shortening that it cannot be detected by measurement; but it must not be forgotten that in the children especially it is exceedingly difficult to measure very accurately.

"Third. That in transverse fractures, or oblique or denticulated, occurring in adults, and in which the broken fragments have become completely displaced, it will generally be found equally difficult to prevent shortening; because it will be found generally impossible to bring the broken ends again into such apposition as that they will rest upon and support each other.

"Fourth. That in all fractures, whether occurring in adults or in children, where the fragments have never been completely or at all displaced, constituting only a very small proportion of the whole number of these fractures, a union without shortening may always be expected.

"Fifth. That when, in consequence of displacement, an overlapping occurs, the average shortening in single fractures, where the best appliances and the utmost skill have been engaged, is from half to three quarters of an inch" (p. 417).

Measurements are apt to be lightly or carelessly taken, and surgeons sometimes speak in a loose, easy sort of way about "no perceptible shortening" &c. Hamilton's description of the fallacies in careless measurement is very good; and his own conditions of a faithful measurement are as follows:

"The patient should repose upon his back, upon an even surface, with the lower extremities as nearly as possible in the line with the axis of the body, the two wings of the pelvis being in the same horizontal line. A flexible, but firm, graduated tape is to be preferred to the steel tape measure. The foot being steadied by an assistant, the surgeon should put his thumb-nail against the line where it joins the ring and push his nail into the skin just *below* the anterior superior spinous process of the ilium, pressing firmly up and back, the flat surface of the nail resting upon the skin."

In this way he will obtain a fixed point, and he can obtain an exactly corresponding point upon the opposite side. Below the measurement may be made from either malleolus, but the outer has the most defined extremity and is generally to be preferred. In most cases for some months after the termination of the treatment, there is some swelling about the ankle, which renders it necessary to use great care in defining the point of the malleolus. The thumb-nail of the opposite hand may be

used for this purpose, resting vertically upon the skin (flat against the lower end of the malleolus), p. 418.

The extraordinary complexity of apparatus and somewhat misplaced ingenuity of American surgeons are nowhere shown to more advantage than in Hamilton's chapter on Fractures of Patella, with its very full descriptions and drawings of all sorts of machines. No wonder at the end of it he finds it necessary to caution against the too early application of bandages, and to advise the surgeon to see his patient daily, if not twice a day for the first week of the treatment (p. 472).

The curious manner in which the use of Dupuytren splints for Potts' fracture of the fibula has been altered by many surgeons into a misuse which Dupuytren never intended has not escaped Hamilton, and his advice is excellent on the subject (p. 480).

On the very difficult question as to the reason for the much greater mortality after compound dislocation than after compound fracture, Hamilton's observations are most excellent. He finds the explanation in the much greater power the surgeon has completely to reduce and retain in position a dislocation than in fracture and the consequent far greater amount of tension of the parts and especially of stretching of the muscles in the dislocation cases (p. 790).

It would be easy and interesting to multiply quotations and reiterate praises of this excellent book. In comparing it with its classical predecessor in the same subject, we find both written by enthusiasts; the older man with a more polished style and method, but with far less fulness of detail and historical completeness. Could he rise from his grave and see the changes in half a century of progress, he would find few more worthy sons to take pride in than this diligent and shrewd American surgeon.

X.—Mental Maladies and Epilepsy.¹

It appears judicious to associate and analyse together the works of MM. Voisin and Bourneville, and for several reasons. Among these are, that the volumes are contemporaneous, that the subjects considered are of connate origin, and that they record the valuable experience gathered and garnered in contiguous portions of that fertile field for observation, the Hospital of Salpêtrière. But, although these treatises approximate at certain points, and although their authors belong to the same practical and pathological school, and are impelled by the same spirit and stimulus, their productions differ very widely in the range embraced and in the impressions conveyed. M. Voisin's volume is condensed, concise in style, rich to affluence in important original observations, discoveries, descriptions of what he believes to be original. It submits to his readers the most precious facts and deductions accumulated during many years, and subjected to the ordeal of the lecture room in clinical courses delivered from 1867 to 1875, and comprehending certain aspects of almost all the morbid affections supposed to have their seat or sequence in the organs of the nervous system. M. Bourneville, again, is copious, diffuse, minute, confines his inquiries to a limited group of diseases, the title of his volume restricting these to one form of sensori-motor affection, the status epilepticus, although he diverges at great length to other connected phenomena; and while many of his remarks are new or present a novel aspect, his disquisition may be fairly designated as the tribute of an able and enthusiastic pupil to his distinguished teacher, M. Charcot, rather than as a monument of his own genius or industry.

Although M. Voisin cannot be said to have treated the subject of the neuroses systematically, the scope of his lectures is extensive, and includes almost all the great families or genera of mental diseases which nosologists have comprehended under the *Vesaniæ*, and several modifications or varieties of them which he has differentiated from recognised species, and which have sprung up from that rottenness and degeneracy which is known to infect even the best parts of our civilisation. He presents the series in what may be accepted as a pathological arrangement, and the

¹ 1. *Leçons Cliniques sur les Maladies Mentales Professées à la Salpêtrière.* Par le Docteur AUGUSTE VOISIN, Médecin de la Salpêtrière. Paris, 1876.

2. *Recherches Cliniques et Therapeutiques sur l'Epilepsie et l'Hysterie, compte rendu des observations recueillies à la Salpêtrière à 1876.* Par BOURNEVILLE, ancien interne des Hôpitaux de Paris, &c. &c. Paris, 1876.

position thus assumed is noteworthy upon the following grounds :

1st. In the exordium, even before he has entered upon the definition of congestive insanity, insanity from anæmia, he casts a longing anticipative look towards the time of triumph, when, by the aid of increased instrumental means of investigation, such "organic conditions may be demonstrated as the causes of the different mental maladies, and upon a knowledge of which an active and efficacious system of treatment may be founded."

2nd. He declares himself an opponent to the Etiological system adopted by M. Morel, on the pretext of the narrowness of its basis, of its confounding forms of alienation, altogether different, and of creating Hereditary Insanity into a distinct and independent Species, seeing that there are very few forms of madness into which a morbid hereditary tendency does not enter as a cause or complication.

3rd. In discussing Idiopathic Insanity and, collaterally, other modifications, apparently originating in moral causes, he confesses that no lesion whatever can be detected in the encephalon, and that even structural alterations in remote organs may either have escaped notice, or can with difficulty be connected with the mental disturbance.

This is not the arena upon which a controversy can be initiated as to the physical or psychological origin of diseases of the mind ; but, before dealing with the more interesting portions of the monograph of an author who takes a materialistic view of the question, and who boasts—and we concede rightly boasts—that he is the first, or among the first, who has presented a genuine clinical, in other words etiological, symptomatological, and pathological treatise upon the neuroses, it may be expedient to determine precisely what his opinions are upon data which constitute not merely the basis of the creed, but of the practice of psychologists.

After quoting Foville, sen., as an exponent of more precise knowledge, and Moreau de Tours as holding that all cerebral changes are necessarily preceded by a neurose, and reaffirming his own conviction that madness must "always be accompanied by morbid alterations in the brain, he admits" (p. 8) that mania, melancholia, hallucinations, are encountered in various forms and modes of alienation which have no common origin, nor have, in fact, any pathogenic relations. If we meet with mania, &c., and examples are not lacking even in M. Voisin's pages, traceable both to congestion, anæmia, and many other conditions, traumatic, toxic, emotive, our conceptions of causation must either undergo a total revolution, or we may be called upon to concede that we are ignorant of the pathogenetic commencement of all mental

aberration, or that all such moral changes and perversions have a common origin, or that many are without any appreciable pathological origin; in other words, unaccompanied by any structural change. Notwithstanding qualifications elsewhere recorded, M. Voisin declares that necroscopic changes, discernible either by the eye or through the microscope, may be, or should be, seen in every case of insanity. In virtue of this assumption he, with a seeming logic, denounces the irrationality of founding classification upon symptoms; but may we not fairly inquire: Are not morbid changes in cells, so invariably and persistently insisted upon, as infallibly symptomatic of a *previous* condition as mania or melancholia are symptomatic of the morbid condition of the cells? May not such grave perturbations as mania, &c., be the primitive state of change, the first step to unhealth, in other words, the pathogenic origin of the structural alteration? It is certain that mental depression, mental labour, and exhaustion, &c., cause Emaciation and pathological changes in remote tissues, when there is no proof of mental disease or perversion; why, then, should they not arrest or pervert the nutrition of the cells in the cortical substance? It is likewise certain that when no precursory or preparatory physical affection existed in the cerebral substance, sudden fear or joy has established perversion or obscuration of the intellect or sentiments, and after prolonged years of darkness and delirium these have passed away as suddenly as when created by the original shock. To such propositions, which involve the truth that madness may pre-exist cell change, may induce this very alteration, we are entitled to claim the allegiance of the writer, who, at page 73, represents that errors of vision, due to palpable disorganization of the eye, may inspire fear and actual mental disease, or that erratic rays of light, referable to the morbid state of some part of the visual apparatus acting upon the retina, may cause the transmission of false impressions or irritations to the central organ, and thus induce diseased perceptions. As an illustration alike of the difficulties opposing precise pathological investigation, and of the assistance afforded by the microscope, M. Voisin states—and the observation is in itself important—that in the brain of the idiot, apparently perfectly healthy, he has been able to detect considerable lesions, especially advanced fatty degeneration of the capillaries, granular, fatty, and pigmentary alterations in the cells. In vindicating the powers of the microscope and describing the features which it discloses, our author (p. 14) gives the pathognomonic signs of anæmia as common to lypemania, hallucinations generally, and particularly those of smell, and, at page 16, greatly widens this etiology by announcing that hallucinations of vision, depending upon cata-

ract and glaucoma and diseases of remote organs, may be removed by operation or suitable treatment. At a more advanced stage glaucoma reappears as associated with ideas of persecution, which are, however, found to be connected with various and altogether unconnected lesions, such as anæmia and ischuria, local or general affections of the organs of reproduction, &c.

Mental affections are distinguished into the following orders : 1. Acquired insanity, where the symptoms appear after the intelligence has been developed and under the pressure of external events ; 2. Intrinsic, where the malady appears in early life and is dependent upon heredity ; 3. Where it depends upon alcohol or other toxic influences ; 4. Congenital, including cretinism, idiocy, imbecility and various mental defects such as enfeeblement or abolition of the will and understanding, and where there may be or may not be malformations of the skeleton ; 5. General paralysis, in which the symptoms and lesions have been traced with great precision, and traced in relation to the present state of our physiological knowledge ; and 6. Senile dementia. From the sub-genera, or we should rather say, the subdivisions of these grand classes, and from the collateral considerations in which the author indulges in discussing these, there will be selected seriatim whatever is most novel, striking, or characteristic of his investigations and conclusions. Of the forms included under acquired alienation, sympathetic or secondary, where the derangement is consequent upon epilepsy, hysteria, &c., sensorial and idiopathic, or primitive, our exposition shall at present be confined to the latter. This form may confessedly exist where no appreciable change in nervous structure can be traced, and where such lesions can be traced. Under the first category are cases originating in grief, intellectual exhaustion, disappointed love or ambition, religious impressions, where moral causes predominate or are exclusively present ; the attack is often sudden or instantaneous, but it may be fairly contended that this abrupt interruption to health may have been preceded by a period of incubation, by imperfect nutrition, by what is defined as "spasm of the small vessels;" by congestion or anæmia depending either upon diminutions of the globules and change in the quality of the blood, or upon that impoverishment of the fluid consequent upon atheromatous degeneration of the arteries, &c.

He protests against the theories of M. Baillarger that congestion is inflammatory in nature, seeing that it may be an adjunct of the presence and pressure of intra-cranial tumours, and that it may be ranked as the first stage of general paralysis. He likewise insists upon the necessity of distinguishing between the products of congestion and congestion itself, because the

latter may arise at and in the moment of dissolution and prove transitory, whereas the former are constituted by the transudation of blood through the walls of the vessels ; but we cannot here follow the author further in his sketch of the pathological changes of the blood-vessels of the brain.

In depicting the external features of congestive insanity the pupils are said to be of equal size and contractile ; the grave physiognomy to be seldom disturbed by laughter ; for the delirium is characterised by ambition but not exhilaration, the imaginary riches are neither enjoyed nor boasted of. This portrait is drawn in order to show the distinction between the malady and General Paralysis. Another difference between these two diseases is, that in the former the memory remains unimpaired, correctly reproducing not merely the events of early and healthy life, but those which have occurred since and during the invasion of mental disturbance. On examining two of the typical cases detailed at great length, omitting all negative and non-essential evidence, the following changes were detected by the microscope in the first : 1. Softening of eighth pair of nerves ; 2. A mass of small granules alongside, or else found in the neighbourhood of the olivary bodies, and prolonged to the fourth ventricle, the walls of which they cover ; 3. At various points of the fissure of Sylvius the arachnoid is thickened, resistant and opaline ; 4. The grey centre of the sphenoidal lobe of dark colour and arborified ; 5. The left hippocampal convolution presented the same appearance ; 6. Red punctæ in the left quadrigeminal bodies, lacunæ and depressions between the geniculate bodies (p. 122). Whereas in the second case there were : 1. The sheaths of numerous vessels of one parietal convolution were infiltrated with fatty matter, while others were gorged with blood, and the cells were infiltrated with yellow or orange fatty granules, but there were no proliferations of the connective tissue ; 2. Dark points and considerable dilatation of a vessel in the right sphenoidal lobe, which was likewise engorged with red globules at the same place and exhibited masses of coloured hematine and fatty cells ; 3. Masses of hematine or hematosine at the bifurcation of certain vessels ; 4. Globular extravasation in optic tract ; 5. There were also lesions in the sensorial centres and parietal circumvolutions (p. 25). It was expedient to contrast these descriptions both as demonstrating the minuteness of the pathological observations of M. Voisin, the shortcomings of modern microscopic anatomy, the irreconcilability of the pathological changes in examples of the same morbid process, and the impracticability of founding any system of classification upon such changes in which the mental phenomena shall be at all considered or represented, seeing that both of the patients in whom

the changes recorded were observed,—differing as these do so widely in nature and number,—laboured under the Mania of Suspicion with delusions as to persecution, and in one case hallucinations. Great importance is attached in this inquiry to the diminution of phosphorus, as determined by chemists, in the cerebral matter, as its presence in normal or supernatural proportion is supposed to be connected with the activity of the organ. How far the stimulating quality of this inflammable substance may have suggested such a theory, it would be vain to inquire. Marcé is subsequently quoted as having shown that dementia is consecutive on degeneration of the optic track; Demme as having observed that many individuals suffering from chronic affections of the eyes become insane; and M. Voisin confirms this observation, adding that he has become acquainted with a great number of examples where alienation was consecutive upon feebleness of the external senses. The frequency of lesions in the optic track in congestive insanity and the supposed effects of these in exciting and perverting the operation of the perceptive powers, when the patient is sensible of actual impressions but incapable of estimating their nature or distinguishing them from such as are preserved by memory, has tempted M. Voisin to make the fanciful calculation that, under the excitement of etherisation the force of an impression equal to one in the sane is equal to ten in the lunatic.

What is called passive congestive insanity is cursorily alluded to; it is attributed to spasm of the vaso-motor nerves or a stasis in the capillaries, and is conceived to be connected with anæmia: atheroma, in a high degree, and a fatty web involving the whole of the walls of the vessels, which contain hematine, hematosine and carbonate of lime.

The therapeutic means of combating these conditions are stated to consist in small bleedings, the application of blisters or cautery to the nape of the neck, and other antiphlogistic and revulsive applications; and, secondly, in the use of digitalis, *Veratrum Viride*, and Hydrotherapia, in order to lessen or lower the rapidity of the circulation.

The second section is occupied with the subject of insanity dependent upon simple anæmia, which it is affirmed is due pathologically to a quantitative or qualitative alteration in the blood, whereupon excitation of the cells by insufficient nutrition follows, signalised by depression and by simple melancholia with agitation, rather than by the type of mania with excitement. The author, in propounding the doctrine that anæmia is the sole cause of chloro-anæmic insanity, is opposed to the opinion of Lelut that this disease is principally to be referred to moral causes, and he thinks it necessary to expend an undue amount

of argument in support of the general proposition that this condition is the fertile source of large groups of mental perturbations. Thus, he says, it has been long and almost universally admitted that sorrow, suffering, poverty, privation, innutrition and inanition, and all sapping and suspension of vital force are the main factors of depressing emotions, and delusions of sight and hearing, eventuating in sombre and saturnine conceptions. The loss of tactile sensibility in such cases, and their marked curability, are brought prominently forward.

Passing legitimately to certain of the intimate causes of anæmia, he claims originality in having pointed out atheroma as the chief of these, contending that while previous observers established a connection between such degeneration and the predominance of our passions and propensities, and between cardiac and encephalic disease, he first traced the course of the latter, even when due to moral causes, up to arterial degeneration and impoverishment of the blood, mental deterioration with ulterior. Whenever the walls of the larger vessels undergo such an alteration, it may be predicated in the smaller branches, as well as the absence of red globules, the exsanguine state of the nervous tissue, and the presence of fatty granules in the cells of the cortical substance. This last, moreover, is rusty in colour; its tubes are shrivelled, emptied of their myeline, the communicating canals are filiform, and there are likewise hematine exudations from interruption to the circulation. These changes being almost identical with what have been observed by Luys and Marcé in senile dementia, the attempt is made to reconcile the discrepancy by recognising atheromatous degeneration as that disease coincident with adult age, and the significant and impressive remark is added that such morbid affections are to be measured less by the number of years during which the patient has lived, than by the "fastness," the excitement and indulgence which have marked their passage. Thus atheroma has been found in a boy of fourteen subject to rheumatism, and in patients where General Paralysis has followed profound dejection and fatigue at an early age, and particularly in one where gangrene of the skin of the abdomen was traced to general atheroma, and locally to embolism. It is evident that this state of the walls of the arteries interrupts nutrition and disturbs the functions of the nervous matter, and, by creating changes in the cells, vitiates sensory impressions, and thus induces illusions; results which are not to be confounded with primitive illusions, or those arising from the state of the external and internal organs of sense. During life atheroma may be diagnosed by the state of the heart and carotid, by the presence of the arcus senilis, by the flexuosity of the

arteries of the retina, and by the increase of pulsations during pressure on the contents of the orbit. In one phase illusions precede delirium; in another delirium is the first symptom. The sufferers are idle, ruminant, frivolous, rarely curable, from the inefficacy of all remedies in atheroma; but although they in many respects resemble the victims of senile dementia, the memory remains intact; and while fatty degeneration is contemporaneous with the incursion of senile dementia, it does not appear till the close of the disease now under consideration.

As a contribution towards the natural history of General Paralysis, we are furnished with speculations on the connection of the disease with posterior spinal Meningitis, and the consequent production of paresis of the lower limbs and of symptoms resembling those of sciatica, as discovered by Westphal. The appearance of vomiting, shivering, halting, trembling, oscillation of the lower limbs, with pain on pressure on the cervical or dorsal vertebræ, are pointed out as phenomena indicative of progressive increase of the inflammatory or sclerotic affection and of evil import. This progression may initiate epilepsy, neuralgia, and other aggravations; and in fact neuralgia may sometimes be accepted as the prodrome or commencement of the long series of symptoms named General Paralysis. As noted in the three stages of this fatal disease, the pathological appearances are as follows:—

1. Marked vascularity of the pia mater and arachnoid covering the medullary axis, and transparent miliary granulations the intimate structure of which consists of an envelope of connective tissue, vessels with thickened walls, and a great number of empty nuclei in process of proliferation.

2. Increased thickening of membranes, loss of transparency in arachnoid, which presents numerous opaline spots, and is closely adherent to the pia mater. There is likewise disappearance of the subarachnoid spaces, whilst a sort of sheath or cylinder is formed around points of attachment and roots of nerves. The pia mater is likewise attacked with inflammation.

3. Degenerations of the arachnoid, consisting either of soft masses of nuclei in proliferation differently distributed; secondly, of numerous small rough disseminated bodies of a fibro-cartilaginous nature; thirdly, of these bodies undergoing a calcareous transformation, presenting during the change numerous layers of fibrous connective tissue. In a more advanced stage bodies are seen resembling osteoblasts, such as those encountered subsequent to inflammation of the serous membrane of the abdomen. These corpuscles, apparently of different ages, may be met with at various points, and very numerous attached to the vertebral Theca, as well as in the visceral arachnoid.

The microscopic anatomy of the parts more intimately involved, to which the characteristic symptoms of the disease are chiefly referred, is reserved for a chapter on the imperfections of language which mark the progress of the affection. This examination leads to the general conclusions: 1st. That the drawling, hesitation and *annonnement*, or the arrestment and gaping which interrupt articulation and appear to be formulated to a prolongation of the letter A, are due to lesions of the cerebrum; while stuttering, stammering, and tremulousness are connected with lesions of the bulb, but that mutism may depend upon changes in the cerebrum, and changes in the muscles of the nerves of the tongue, lips, &c. There is an effort to connect the multiform errors of speech which have been recorded with certain special or localised alterations, and so far as this physiological experiment appears to have been successful, it shall be here epitomised. The loss or impoverishment of language is first referred to affections of the cells of the cortical substance of the anterior lobes and of the insula, and which suspend, we cannot tell how, the origination of the idea to be represented, or the sign with which it is to be associated. The structural changes enumerated are abundant embryoplastic nuclei in the vessels, the walls of the vessels or the perivascular sheath; and throughout the grey substance these bodies, coming either from the vessels or, according to Magnan, from the neuroglia, are met with. Besides these, atheromatous deposits are met with, especially where the patient is aged. But supposing that no impediment, arising from structural or other lesion, should exist in the cells of the periphery, obstacles or complete obstruction to the transit of the exact idea or its sign may be created in the course of those fibres which communicate between the anterior convolutions, the medulla and the organs of articulation. These obstacles have been attributed in certain cases to enormous masses of nuclei and embryoplastic particles in the course of the fibres, but especially in the perivascular spaces and vascular sheaths; likewise to hematosine and yellow or discoloured hematine occurring in extravasations and points of softening distributed in the bulb and the corpora striata. But supposing that the message had been faithfully transmitted unopposed and uninhibited by any imperfection in the facial, glosso-pharyngeal, hypoglossal, or other conductors from the bulb, morbid alterations may have taken place in the nerves supplying the tongue and lips, and then the message will be arrested in the very act of articulation. The actual alteration our author claims to have discovered to be fatty degeneration, and he distinguishes the defective utterance as somatic, and as, in its origin at least, not paralytic. He asserts that in chronic

cases, and where the accentuation of the speech was very marked, he estimated the number of diseased to healthy cells in the nucleus of the hypoglossal nerve as four to five ; and even in a recent case, where the disease had run its course in twenty-five to thirty days, the proportion was as one to five ; but the embryoplastic nuclei were detectable only in the vascular walls and perivascular spaces. The association of healthy with unhealthy cells, the disappearance of the latter, and where they do not disappear the probable discharge of their functions by the former, serve as an explanation of the recoveries which take place in infantile paralysis, and of the temporary suspension of stuttering, and even of the morbid mental phenomena in general paralytics. Combating the conclusions of Van der Kolk, that mutism and vices in pronunciation are connected with changes in the olivary bodies, M. Voisin is disposed to limit the tremor of the lips, tongue, &c., to local ataxic causes, and he denies that it can be regarded as pathognomonic of general paralysis, as similar impairment may be noticed in alcoholic and mercurial poisoning, in old age, and as a congenital defect. It is not a new, but an interesting observation, that the nerves engaged in expression become involved in the general decay, that the face assumes a still, stolid aspect, and that even in general muscular acts there may be witnessed hesitancy, interruption, uncertainty, which is extended to writing, where agraphia takes the place of mental aphasia, as bad spelling, alienation, and syllabilisation, and repetition of words constrain the hand as well as the lips of the general paralytic.

In a section the most prominent object of which is to reiterate his conviction that every form of alienation is somatic, and that moral means are insufficient to arrest the physical ravages, —cases of lypemania are narrated, in which lesions of cerebral cells and capillaries and localisation of such in the parietal convolutions were chiefly remarked. Besides such lesions there were also apoplexies, effusions of hematine and hematosine in the lymphatic sheaths, ampullaceous dilatation of the arteries, death of certain vessels, atheroma, and diminution of the quantity of phosphorus, all likewise characteristic of this class. In the careful examinations, which strongly corroborated M. Schiff's experimental results, it is shown that external impressions, especially upon the special senses, produced exaltation of the temperature in different parts of the nervous centres, and in respect to the influence of vision, hearing, &c., in a special degree that of the parietal convolutions, and this altogether independently of acceleration of the circulation or any other excitement.

He holds further that all sthenic stimulation, whether through the external senses or other sources, operating through the physical organisation, induces anæmia and dyscrasis of the nervous centres and system, manifested in the cells and fibres of the peripheric and medullary cerebral matter, and in the ganglionic system. The mental conditions connected with these anatomical changes are chiefly lypemania, with or without stupor, followed by abstinence, emaciation, cachexia, and exhaustion. Practically he has noticed the presence of leucocythemia in such patients; theoretically he has inferred that when attacked by typhoid fever, paralysis of the vaso-motor nerves of the cerebrum ensues, or the sthenic state of the arteries is relieved, and delirium is mitigated and ceases. Under the title of sensorial are comprehended all kinds of unsoundness which depend upon the conditions of the external organs of sense. These are imagined to be extremely amenable to treatment. It may be considered altogether hypothetical to affirm that irritation or hyperæsthesia of the apparatus of sensibility are the causes and not the outcomes, the symptoms or concomitants of cerebral disturbance; but such conditions may be conveniently grouped together as connected with a particular set or series of phenomena, and as throwing some light upon the etiology of physical deviations from health. The views of Baillarger, who conceives that hallucinations arise from sensorial impressions, altogether independently of extreme excitation of the external senses; and the assumption of Griesinger that mechanical irritation of the retina and of the auditory nerve produced in the former merely flashes of light, in the latter merely tinnitus and not precise forms or words, are rejected; and the conclusions of Michea and Morel, that the origination of primitive hallucinations is to be found either in the peripheric nervous matter or in the nerve trunks connecting that with the encephalon where the elaboration and completion of the erroneous image is perfected. The adoption of this opinion is supported by the evidence afforded in unilateral sensory deceptions, as where spiders, spectres, sepulchres were only perceived when the right eye was shut and the left open. In the experience of Dumont, physician to the Hospital for the Blind at Quinze-Vingts, who of 220 patients found 27 labouring under various degrees of intellectual disorder, it was found that the operation for double cataract removed not only blindness but amnesia, incoherence of ideas and defective spontaneity, and that the same result followed supervention of total blindness. Lastly, of twenty-eight inmates of Salpêtrière, who presented primitive but not secondary hallucinations, and in whom the organs of vision were affected,

two had glaucoma, and eighteen opacity of the crystalline lens. The accuracy of the latter results was tested by the celebrated ophthalmoscopist, Dr. Galezowski. Such errors may be distinguished from psycho-sensorial hallucinations by the fact, that while becoming intensified in twilight they cease in profound darkness or when their eyes are bandaged, and that although aggravated by bustle and noise, the sufferers are rarely excited, and are so quiet and docile as to engage in active occupation. It appears possible, however, that even illusions of flies, spiders, and small objects, though recognised as such, may so irritate and annoy by their incessant presence and petty persecutions as to pervert and colour the mental perceptions to which they give rise. But, where there is no disorganisation of the intra-orbital tissues, there may be excitement and acuteness of their functions, so that flames and different degrees of luminousness disturb vision or even perception. These and many other morbid sensations are affirmed to be controlled, even cured, by the hypodermic injection of morphia. Similar remarks are applicable to hallucinations of hearing, especially as to their cessation on the loss of audition, the occlusion of the ear and during silence.

Where the nerves of common sensation are subject to neuralgia, malaise, modifications in the feelings of weight, resistance, temperature, and so on, and where these form symptoms or complications of insanity, the complexion of the delusions is that of suspicion or apprehension, and the sufferings or annoyances are attributed to enemies or external circumstances. Sometimes the pain may occupy the space of a few inches, but its influence is extensive and important; and an example is cited where a disposition to bite was suggested by neuralgia under the jaw, and another where a painful point between the epigastrium and umbilicus created the belief that there was an animal in the belly. Such perversions of touch are sometimes assimilated to an aura, appear to rush suddenly and swiftly from the extremities to the pelvis, head, and so forth, first exciting extreme anguish, cries and agitation, and, ultimately, hallucinations. When the ganglionic system becomes implicated, sensations of hunger, distensions of the abdomen and bladder arise, and constitute many of the characteristics of lypemania. All these, even when they amount to delusions as to the magnitude of the body, as to the action of electricity, and as to blows inflicted by unseen and unknown persecutors, have been found to yield to the employment of the hypodermic injection of morphia.

Under the designation of sympathetic insanity, which we regard as open to grave objections, are comprehended or con-

founded, all nervous maladies which are not intra-cranial in their origin or progress, but which are dynamic or connected with changes of distant organs, and those which depend, directly or pathologically, upon the sympathetic nerve or its ganglions. The latter class may include the mental effects of gestation, of remote tumours, or of alterations in the nervous matter not appreciable perhaps to the naked eye, but detectable under the microscope. Two typical illustrations of this sort are described at length. In the one there were the ordinary indications of hypochondriacal madness, and the delusions that the patient was daily the mother of thousands of infants, who furnished animal food for the community. In the other, to exalted general sensibility and intentions of suicide, there were added refusal of food, abdominal hyperæsthesia, and hæmorrhages, and ultimately death by inanition. The narrator, however precipitate may be his conclusion, after discussing fairly whether the morbid impressions from the seat of disease reached the seat of consciousness directly or indirectly, conceives that the transmission must have been by reflex action, and this upon the data furnished by the autopsy. The abdominal ganglions of the sympathetic appeared to the eye perfectly normal, but under the microscope there were observed numerous embryoplastic nuclei passing into fusiform bodies, and new lamellated tissue running in various directions. There was a diminution or atrophy of the nervous cells, but such as remained were filled with pigmentary or fatty granulations, and of some the nuclei had altogether disappeared, whilst the envelope was greatly thickened. When it is known that at least in one of the patients under consideration the peritoneum was covered with lymph and the intestines, liver, and other viscera agglutinated together, that the former were at one point contracted, and at another enlarged in their course, and that the transverse colon had been diverted from its ordinary situation, and occupied a vertical position—precisely the appearance to which Esquirol was accustomed to refer melancholia with cœnesthetic delusions—our conviction is that the theory of the author is either invalidated or its confirmation must be postponed. It seems probable that the state of the semilunar ganglion was due to the same morbid process which left such very palpable traces in the abdominal viscera, and that in this process, rather than in its pathological consequences, is to be sought the pathogenesis of the psychosis. Lobstein again may be quoted as corroborating this conjecture, as he records alterations of the sympathetic after diseases of the intestines, and, as the last member of the series, mental derangement. It would appear that Pinel invariably referred hypochondriasis to the sympathetic nerve;

that Bichat associated the semilunar ganglion fibrous with the same affections; and that Davaine connects maniacal attacks in children with the presence of worms in the intestines; and that mental perversion of every possible variety has followed or appeared to follow change in every organ engaged in digestion and nutrition, according to the experience of De Lucé, Mathez, Boulton, Guislain, Marcé, and Schroder van der Kolk. M. Voisin, notwithstanding, very candidly confesses that Stahl, Cullen, Falret, and Georget are opposed to his doctrine, and have argued in favour of the proposition that the structural degeneration or alteration even in remote organs was secondary, in other words the consequence of the mental excitement and aberration.

Tubercular insanity is described as, psychically viewed, melancholic delirium, as marked by deep depression impelling to suicide, rarely ideas of riches or power, and hallucinations exclusively of sight; physically viewed, as tubercular meningitis extending to spine, with deposit of tubercle in the lungs and bones, and attended with paralysis of the face and extremities. These deposits are met with in the pia mater, either in a miliary state or of the size of peas, or as granules in the cortical substance; the size perhaps standing in relation to the duration of the disease. These morbid appearances are somewhat assimilated to those in general paralysis; where, however, tubercular granulations are to be found in the capillaries but not in the cortical substance. A resemblance between the mental manifestations in tubercular insanity and those in general paralysis and simple monomania has likewise been traced; but how widely these forms are separated may be gathered by the following detail of special indications in the former:—ptosis, paralysis of the face and arms, cross-hyperæsthesia of right leg and left arm, spinal and epigastric pain, increased and irregular action of heart, syncope, vomiting, night fever and sweat, variable exacerbations, during which there is dilatation of the pupils, amblyopia, inability to distinguish colours, frequent abscesses, fistula, caries of the bones generally, and especially sores on the elbow, with affections of the olecranon. Arrest or amelioration with dementia, but not recovery, is to be looked for in such cases; and their mortality may be imagined when with the cerebral disease there are conjoined chronic laryngitis, degeneration of the spinal cord, and solidity or softening of the lung. The author does not regard this malady as a mere expression of phthisis, nor as a mental condition progressing simultaneously with lung change; but as distinct and independent, and associated with tubercular degeneration wherever that may occur; and in support of this opinion quotes Louis' momentous axiom that wherever tubercle affects the lung it

exists in other organs. In confirmation of this postulate the experience of M. Guislain is given, who mentions a patient being seized with phthisis after labouring under melancholia with stupor for four years, who recovered from her insanity; an alternation which took place on three successive occasions. Support is likewise sought in the explanation advanced by Schroeder van der Kolk, that both the pulmonary tubercle and the mental disease are the consequence of excessive excitability of the bulb and of the pneumogastric nerve. It may be a sport of fancy in M. Voisin to conceive that the researches of MM. Grancher and Thaon upon the origin and nature of tubercle justifies the theory that those affected with tubercles would have become general paralytics had not the cerebral inflammation determined the deposit in appropriate regions.

There follow several lectures, which are not, but might fairly be, named appendices, as they cannot be said to fall under the programme of the author, and would appear as excrescences upon any systematic course. Of these, we shall say nothing of that upon communal insanity, as it was fully discussed in our No. 107, for July, 1874, nor on that entitled Alcoholism and Absinthism, as the subject was similarly treated in our No. 106, for April, 1874.

We must confine our reference to the fresh and fascinating investigation on juvenile insanity, first, to the period assigned for its appearance, being between four and eighteen years; second, to the causes which are common to insanity at all ages, and special, as comprehending masturbation, harsh treatment at school upon susceptible natures, deficient nourishment, epidemicism as seen in the Children's Crusade; thirdly, irritation, as in the visionaries of Salette and Lourdes; and, fourthly, to the characteristic symptoms of violent passion, theft, inebriety, abstinence, attempted suicide, mutism and stupor; and would, lastly, suggest to those interested in the prosecution of the inquiry an admirable and exhaustive article, p. 284, No. 33, of the 'Journal of Mental Science' for April, 1860. We would conclude this review by three observations. First, that great merit should be awarded to M. Voisin for having prepared an ample digest of what has been observed, and particularly what he himself has observed or tested, in the department of pathological anatomy as connected with morbid mental phenomena. Secondly, that, although perhaps naturally disposed to regard the disclosures by the microscope as discoveries, and to elect them, not as the causes of mental disturbance generally, but of special forms of mental disturbance; in fact, to materialise or localise what are still regarded as subjective states, he has throughout displayed a spirit of caution, wise

deliberation, and fairness. And, thirdly, that it cannot escape the notice of even a cursory critic that such a spirit was, and will for some time be, imperatively necessary; as precisely the same marks of degeneration in nervous substance, such as fatty degeneration, miliary aneurisms, disruption or destruction of cortical cells, exudations, atheroma, discolorations, &c., re-appear again and again on the field, do duty in a manner under various and incompatible conditions and circumstances, and are associated with discrepant mental affections, which, whether they have a common origin or not, are neither identical, nor allied, nor convertible.

As M. Voisin has scarcely alluded to epilepsy, nor the proteiform sensori-motor maladies which may be said to be engrafted on that main trunk, M. Bourneville's essay may be accepted as a suitable supplement; but, although the title at first suggests the idea of specialisation and insulation, the author travels over a very long and large space. Thus, he starts with an account of the Status Epilepticus, passes over its symptoms, treatment, and management, and arrives at the history of the extravagant, almost incredible, and grotesque distortions of Hysterio-Epilepsy, as exhibited in slight convulsions, or in the curious automatism of crucifixion, which is rarely, if ever, witnessed in this country, probably in consequence of the absence of objective aids and accompaniments to our worship. L'état de mal epileptique is said to be marked (1) by attacks of convulsions which succeed each other without interruption; (2) by different degrees of collapse reaching to absolute coma, without any return of intelligence; (3) by hemiplegia, more or less complete and temporary; (4) by acceleration of pulse and respiration; (5) by elevation of temperature, continuous during the paroxysms, and interval, and increasing after their cessation. This latter conclusion is supposed to invalidate that by Dr. Clouston, as to a fall of temperature previous to the incursion of each fit. In a narrative given as typical, the patient, a female, became affected with the petit mal at five years of age, when, after vacant laughter, she fell rigid, but without convulsion, recovering consciousness in a minute or two. At eleven she became hemiplegic with permanent contraction of muscles of foot and loss of power of prehension in the hand. Until fifteen, attacks of stupor were frequent, those of epilepsy rare. Squinting and immobility of facial muscles were observed at nineteen; the two sides of the body were symmetrical, but the fingers of the left hand were shorter than those of the right, the heat being less on the same side. In the previous year the patient sustained forty-six paroxysms of epilepsy, and 749 attacks of petit mal. Having been placed in Salpêtrière the Status became developed after a residence of

two years, when, in the course of three days after this occurrence, she succumbed. This woman was noted to have had 168 convulsive attacks in twenty-four hours. About six hours previous to dissolution improvement was observed to the extent of diminution of the coma, remission of the accessions, and partial lucidity. Complete coma, stertor, and icteroid coloration of the skin immediately preceded death. The treatment consisted in ice to the head, sinapisms, purgative and quinine enemata, and the application of six leeches behind each ear; and to the last named means is attributed signal though transient amelioration, indicated chiefly in the decrease of the fits from 122 in eight to twenty-two in twelve hours. The examination of the cerebrum revealed want of symmetry between the hemispheres, patches of violaceous marbling of the convolutions, a spot of apparently cicatrised softening of the cortical layers, cavity in occipital lobe lined with membrane containing serous fluid, but not connected with the ventricles, thickening and darkening of the pia mater, of the bulb, inequality in size and shape of the sphenoidal lobes; disorganization of the left hemisphere, so great that it was impossible to recognise the different parts, although perfectly formed convolutions were traceable in its substance. M. Bourneville calculates that in five cases out of eight the Status Epilepticus terminates fatally, that there are two well-defined stages in its progress, the convulsive and the meningitic; that the great and abiding elevation of temperature is the next noteworthy symptom of the whole series, as it does not seem to correspond to the accessions or the remissions.

From a very extended analysis of the comparative efficacy of different drugs and therapeutic measures in epilepsy whether ending in the status or not, the inference deducible is that the continued employment of the ammoniacal sulphate of copper has been attended with benefit. The salt has been given for many months continuously, has been exhibited to the extent of sixty-three grains in five months, its presence in the liver after death proving that the system must have been under its influence. It is recommended to be used in the form of a pill. Leschandelier and Greding derived no such benefit from the use of the acetate, as was vaunted by Veisman. Under the use of the sulphate Herpin reports that of twelve cases only three can be styled recoveries, and even in these relapses had occurred. M. Baillarger succeeded in moderating the violence of epileptiform paroxysms, and indeed in suppressing them, even where the points of approximation to a fully developed access consisted in loss of consciousness, clonic convulsion of a limb, partial cutaneous anæsthesia, alteration in the features, with hebetude of the intellectual faculties. M. Bourneville's own experi-

ence has been limited to relief from the medicament in five cases.

In recommending that the monobromide of camphor should be fully and fairly tried in this disease, M. Bourneville guards his laudation by remarking that the patients under the care of M. Charcot are all aged females, suffering from chronic or long established forms of the malady, that the doses being tentative were too small, and that similar caution regulated recourse to all the remedies under consideration. The quantity at present administered is from one to two grains, or even more, and this quantity, in the opinion of the writer, should be persevered in for a longer period than has hitherto been done. Nine cases are alluded to as having been subjected to the treatment proposed; but the general results recorded are, that while there was an obvious diminution in the number of the vertiginous attacks, there was a less marked decrease in that of convulsions; that the salt has not yet been sufficiently tried; and that the writer entertained strong and sanguine expectations as to its usefulness. It is acknowledged that the use of ice in the reduction of heat and, perhaps, of inflammation, has been long resorted to. Dr. Chapman is mentioned as having brought the application of cold to the spine in various sthenic nervous affections, and particularly in epilepsy, especially under the notice of the profession. This agent in the hands of M. Charcot has been chiefly directed to relieve hyperæsthesia of the ovarian region, on the ground of its power in producing anæsthesia; and in cases of epilepsy complicated with cardiac disease and rapidity of pulse, on the ground of the benefit derived from its use in aneurism of the aorta. In one instance the pulse was lowered to fifteen and twenty per minute, in others the normal reduction of the circulation has been permanent. Benefit has been derived from such applications in angina, or where agonising pain occupies the place of an aura, but these act a more prominent and important part in the diminution of vertiginous feelings, which have been long recognised as sapping more ruinously the fabric of mind than even violent convulsions. The author does not appear to dread the supervention of syncope in the employment of ice in cardiac epilepsy, but signalises very sensitively the difficulties which are presented by the restlessness and resistance of patients.

Oxide of zinc seems to have been recognised as possessing curative properties, which, however, did not secure uniform success, since the time of Paracelsus and his imitators. Herpin exhibited the medicine in conjunction with the Extract of Valerian in forty-six patients. The administration was sometimes followed by disorders of the stomach and bowels. These acci-

dents, the intractableness of the cases treated, the reluctance of the patients to take a nauseous drug, and the partial benefit obtained, alleviation being reported in only two of the four cases experimented upon, rendered the agent unpopular in the eyes of M. Charcot, who substituted an inert powder for valerian, so that no error could arise as to the source of benefit.

The introduction of the nitrate of amyl as a therapeutic agent in epilepsy, and especially in the Status, is comparatively recent, and appears to have been determined by a consideration of the physiological effects of the drug upon man and animals. It is certain that the inhalation of the substance is attended almost instantaneously by an acceleration of the circulation and by its subsequent fall to or below the natural standard; 2ndly, congestive flushing of the neck, head, lips, throat, a coloration which passes from red to blue and from blue to great pallidity. The same intensity and variability of hue has been noticed over the whole of the superior parts of the trunk, and in the capillaries of the retina in epileptics, by Dr. J. Crichton Browne, and within the cavity of the skull in rabbits by Drs. McBride and Kempster, and interpreted as an index of the dilatation of the arterics; a notable and prolonged fall in the temperature, which subsequently rises to the same degree as at the commencement of the experiment, has likewise been noted. It is unnecessary to discuss here the speculative notions of Drs. Brunton, Pick, and Steketee, that Nitrate of Amyl exercises a toxic influence upon the muscles; of Amerg drog, that it acts upon the peripheric ramifications of the nerves or directly upon the muscular fibres of the vessels, or the opinions of those who conceive that it may exercise some power over the constituents of the blood. It will suffice to state that the majority of physiologists conceive that this anæsthetic acts upon the walls of the blood-vessels. This opinion has been boldly formulised and advocated by Dr. Crichton Browne, who, regarding the pallor in epilepsy not merely as a precursory sign but as the first stage of the paroxysm and indicating the presence of congestive tension of the arteries, conceiving that if, by nitrate of amyl or other means, these vessels could be dilated and their tendency to contract opposed, and the central vaso motor, to which he attributes the spasm, be thereby and to such a degree affected, the convulsive paroxysm would be arrested or interrupted.

This authority while conscious of the great gain of preventing the recurrence of a single fit by the suspension of a pathological habit has directed his attention chiefly to the efficacy of nitrate of amyl in the Status Epilepticus. In the West Riding Lunatic Asylum Medical Reports, vol. iii, 1873, p. 153, the histories are given of four patients to whom nitrate of amyl was exhibited

while plunged in profound coma and the gravity of whose condition may be estimated by the continuation of the status for several days and by the occurrence of ninety-one, thirty-four, eleven and sixteen fits respectively upon the day when the fumes of nitrate of amyl were inspired—all the individuals thus medicated recovered, or, to speak more precisely, survived the cessation of the convulsions and regained their previous state of health.

Dr. McBride ('Chicago Journal') records four trials during similar crises in one of which the attempt was abortive, in three the benefit was decided; and in one patient such implicit faith was reposed in the medicine that he continued to carry his panacea in a phial in his pocket and to apply to it successfully whenever the approach of a paroxysm was intimated by an aura. Dr. Weir Mitchell, U.S., seems to be amongst the first to have resorted to the employment of the nitrate of amyl in checking solitary attacks, but he restricts its use to such cases as are preceded by an aura, in other words by such an interval of time as permits of the inspiration of the vapour and prefers such as confine the invalid to bed and occur in a regular series. The benefits observed, though limited, are unequivocal and may be illustrated by the suspension of attacks during nine months and by the arrestment of imperfect seizures. Like Dr. Crichton Browne he has signalled the recuperation of the memory and intellectual faculties under repeated recourse to the nitrate of amyl. In the connate affection of hystero-epilepsy M. Charcot is quoted as confirming the efficacy of this resource, and his experience is supported by a number of striking and satisfactory examples, and M. Bourneville takes upon himself the responsibility of subjoining that the evidence of the utility of the remedy in pure epilepsy is incontestable and that it invariably postpones a relapse for one day.

Dr. Philip ('Journal of Mental Science') was not so fortunate in dealing with a number of chronic Epileptics, in fact no advantage seemed to accrue from his experiments.

In the service of Trelat, after the disappearance of epilepsy, under the operation of the nitrate of amyl, and after a partial return of consciousness and the lapse of five days, death ensued; but there was no light thrown by the post-mortem on the cause of this termination.

As the insane community in Salpêtrière, superintended by M. Charcot, consists exclusively of females, his lectures, as has been stated by his pupil, naturally contain classic and copious details of the phenomena of hystero-epilepsy. This disease, with which we are less familiar in England, is defined as hyperæsthesia of the ovarian region with hemianæsthesia, muscular contraction and rigidity, and the relations between

these different symptoms ; but it may be inferred from the narratives given that temporary but prolonged ischuria is as characteristic as any of the other symptoms enumerated. An amplified description of the development of these symptoms is supplied in two cases which shall be epitomised as affording a correct and complete representation of this morbid condition.

A.—The history of the first patient yields proof that some of the worst features of baby-farming disgrace the boarding-out system in France, where such an arrangement has been hitherto proclaimed a valuable branch of the *Bureaux de Bienfaisance*, accredited as a voluntary and eleemosynary substitute for our well-abused poor-law. The child is placed with a guardian who utterly neglects her ; on the remonstrance of neighbours she is removed, covered with vermin, while the scalp was covered over with crusts, or hollowed out into sores from filth or skin disease ; then transferred to a second who is equally careless, leaves her unfortunate charge tied into a chair while she goes to work in the woods ; and then to a third who performed somewhat more of the duties of a nurse-mother. Uneducated, susceptible, timid, she is, when aged eleven, terrified by the pursuit of a furious dog, and from that time is affected with *petit mal*. Fear, excited by seeing the body of a person recently assassinated, determined a nervous attack, unconsciousness and subsequent aggravation of the fits of vertigo. Assaulted by a robber, convulsions supervene, then paralysis, temporary contraction of the limbs, and a constant state of fear and agitation, but she engaged in occupation whenever her malady permitted. The violence of her convulsions necessitated her removal to *Salpêtrière*. The intensity of these paroxysms required restraint and seclusion, but, escaping from these nude and disregarding the pain and wounds encountered in her transit, she reached the streets, still under the influence of *panphobia*. Stupor, hemiplegia, convulsions, amenorrhœa, were treated by bleeding and apparently lessened by chloroform. Eliminating descriptions of numerous manifestations of morbid action in all the tissues of the body, “all the ills,” in short, “that flesh is heir to,” marking the passage of many years, but with long intermissions, the peculiar and permanent features of the case may be grouped together as ovarian hyperæsthesia, hemianæsthesia upon the left side of the abdomen, hallucinations of the luminous glory and gladness, and the beatitudes of heaven, without impairment of intelligence, clonic muscular spasms which she called “twirlings,” rigidity seen in trismus, dysphagia, &c., and attitudinization, during which she latterly assumed and preserved for hours, even for a day, the positions of crucifixion, states followed by critical epileptic paroxysms, and by the invariable concomitant

of retention of urine. After a severe aggravation of all these symptoms, and the display of others equally anomalous, having had in one year not less than 649 attacks, this patient was suddenly, even marvellously restored to soundness and sanity, with no other sequelæ than the *clavis hystericus*, and exalted or diminished sensibility in the locality of the ovaries.

B.—Succeeding a fall from a tree but at considerable distance of time she laboured under albuminaria with swelling of the legs and abdomen. Typhoid fever and terror eventuated in depression of a year's duration. Precipitated into the fire by her first convulsive attack she was so frightfully burned, and cicatrization so delayed by repeated attacks of erysipelas, that the features of the face were almost obliterated, the teeth of the upper jaw uprooted, and recovery so slow that she could not swallow anything but liquid food for a year. The affliction appeared to suspend the epileptiform convulsions, as no fit occurred for three years. In this process of degeneration cholera with ischuria of eight days' standing, hæmatemesis returning daily for five months, left hemiplegia, delirium, are the next downward steps recorded. We omit more than the mere mention of dysmenorrhœa, ischuria, prolapsus recti, hemiplegia with loss of sensibility on the same side, with neuralgia of the conjunctiva, palpitation; as they appear and disappear more than once and do not bear directly upon the issue. A convulsive attack ensued presenting coma, general insensibility, muscular contractions of face and limbs, the right arm being apparently paralysed, respiration slow; which was followed by others in which the patient suffers from excitability, excruciating pain of the left side of abdomen, impaired perception of colours traced by ophthalmoscope to dilatation of retinal arteries and polyopia, dysphagia which necessitated compulsory alimentation. Paralysis seems to have ceased spontaneously with gradual restoration of the strength of the affected limbs; but resort to the hypodermic injection of atropine was followed first by quiet, subsequently by a sense of suffocation, grotesque grimaces and gesticulations and apparently torticollis, with the delusion that she was about to be choked, and, lastly, by the immediate resumption of volition, locomotion, at first restricted to a somewhat wavering gait, and by a gradual and entire recovery. It would be vain and profitless, if possible, to trace step by step the manifold therapeutic measures adopted, sometimes initiated by M. Charcot in such affections, as they are frequently suggested by transitory and collateral states or symptoms; suffice it to say that while relying much upon the inhalation of nitrate of amyl and upon the application of cold to the left side of the abdomen, he seems mainly to trust to continued pressure upon the same spot or as

he conceives the left ovarium, and is undoubtedly justified in trusting to this measure if we are entitled to draw conclusions from the facts so profusely recorded in M. Bourneville's volume. It is right, however, to recollect that this distinguished physician must entertain sanguine anticipations of the curability of hystero-epilepsy, as he is said to have pronounced a favorable prognosis or rather prediction of sudden cure in such desperate cases as that of B. That we have abbreviated fearlessly and freely the narratives now given may be estimated when it is known that they occupy one half of the work; but we have introduced them here for these reasons: that the disease has received slight consideration from the alienists in this country as a specific psychose, that it has generally been stigmatised and ignored as purely hysterical or as imposture, and that, whether the sufferings of those supposed to be affected be multiplied or aggravated by the imagination; whether they be entirely subjective or not, they are real, intense, demand and require sympathy and solace, and may, according to MM. Chareot and Bourneville, receive alleviation from the resources of medicine.

Bibliographical Record.

Allen's Essays on Vital Physics.¹—A work by Dr. Nathan Allen upon the artificially produced imperfect development of woman, as observed in the New England States, was noticed in a recent volume of this Journal. The opuscula now under consideration are occupied with the same theme and similar trains of thought, widened by the application of the author's views on the destiny of our race, and to all the collateral subjects connected with the formation of a healthy and useful population.

The author's style is redundant, but clear and cogent, and displays so much earnestness and energy as to show that he is animated by a spirit of philanthropy as well as philosophy. His propositions all gravitate towards the basis that the normal standard of woman for propagation is meant to represent, not the highest or healthiest development of any organ or organs, but of every structure of the whole system. However intimately connected with fruitful propagation and natural parturition, the size and state of the pelvis and organs of generation, these must be associated with a robust well-organized frame, and with the healthy exercise of the functions of physical as well as psychical life. Whenever barrenness exists, and whenever the offspring dies early or is diseased, it is legitimate to hold that this is contemporaneous with or the consequence of some defect in structure or irregularity of function in the brain, in the organs of respiration, circulation, digestion, milk secretion, or anatomical relations of the skeleton, as well as in the parts directly concerned in the procreation and production of the species. It is notorious that pregnancy is with some women a period of improved health and robustness, whilst with another and perhaps the largest portion of the sex it is a continuous malady, depending

¹ 1. *The Normal Standard of Woman for Propagation.* By NATHAN ALLEN, M.D., LL.D., Lowell, Mass., 1876.

2. *The Law of Longevity.* By same, 1874.

3. *Hereditary Disease.* By same, 1873.

4. *Lessons on Population.* By same, 1871.

5. *The Intermarriage of Relations.* By same, 1869.

either upon some hereditary infirmity, upon unphysiological education or training, upon the violation of some law of the constitution by indulgence in secret vices, fashionable excesses, errors in diet or regimen, or the presence of some acquired disease incompatible with the purposes of maternity. The changes brought about in the relation of parts by utero-gestation do not necessarily involve either enfeeblement or discomfort, as the process of adaptation, in relieving mechanical pressure, in the redistribution of the circulation, which go on simultaneously in a well constructed and constituted system, restore and preserve the balance and harmony of action which are destroyed in the feeble, the deformed, and the diseased. The latter class is proportionally much larger in certain races, ranks, and states of society than others; among savage or semi-barbarous tribes, and among a well-grown industrious peasantry, child-bearing rarely disturbs the functions or ordinary occupations of the individual, while in the grades of an artificial or luxurious people, where the intellect and emotions are poisoned by injudicious culture and false sentiment, where nutrition is vitiated and impeded by a rich but pernicious dietary, where the mammary glands are flattened and dwarfed, the chest contracted and the pelvis distorted by the requirements of dress, the absence of exercise and muscular training, as in New England, the results are sterility, the non-viability of children, or the multiplication of decrepid, degenerate descendants incapable of sharing in the duties and burdens of the community, and either carrying the curse of deterioration into new channels or extinguishing the family line. The genealogy of morbid taints and tendencies originating in imperfect or defective structure is strongly insisted upon, and although the questions are left undecided as to the mode of transmission, as to whether it consists in a specific virus or in a habit of body, whether in the case of consanguine marriages the evil springs from the mere commixture of blood derived from the same source or from the intensification of similar qualities, the product is said to appear, 1st, in congenital affections such as malformations and idiocy; 2nd, where the germs of positive disease are planted in the fœtus, afterwards to grow and bear fruit; 3rd, where from the weakness or intolerance of the frame, exposure to certain influences is all that is required for the reappearance of the inherited physical, mental, or moral depravity. Even one of the accompaniments of civilisation, one of the supposed triumphs of enlightenment and progress, the increased size of the cerebrum, may militate against the increase of population, where, we mean, there is a normal difference between the size of the progeny and capacities of the mother. Thus, it is maintained that the fœtal head also differs in form and shape; that among a people highly educated the anterior lobes of the brain are larger, and that such change gradually takes place just in proportion to the

advance of civilisation. But there are other checks to the natural increase of a mature and manly people. That criminal practice of infanticide or feticide, which historians abundantly prove to have been a blight upon Grecian and Roman manners and refinement, and as greatly influencing the growth of the community, prevails in modern and Christian times. Nor is the perpetration of the latter atrocity referable merely to the unwillingness of parents to bear the burden and expense of rearing their offspring, but abortion is certainly practised solely to preserve the shape of the mother, to avoid the inconvenience of lactation, and to limit the number of the family; and the effect in America in the gradual decline in the native inhabitants is testified to equally by sensational paragraphs in newspapers, by scientific evidence, and by the statute books. This interference operates not merely by the death of the child, but by impairment of health and prolificness of the mother. It may be further noted that nature calls for the same perfect normal organization, the same high standard of functional activity and regularity in the nursing and rearing of infants, that she does in propagation and parturition. To the application of such facts, which the author dignifies by the appellation of "Law," no one could reasonably demur; but while he has copiously enumerated the departures from health and strength, and the causes which lead to them, we have looked in vain for any definition or description of the nature or amount of this normal standard of the condition or relation of the organs and their functions upon which the multiplication of a viable and virile race is supposed to depend. Confessedly the attempt would be difficult; confessedly there is no abstract notion of health, or it is negatively defined by what it is not; but even had a distinct appreciation of the properties represented by the term been arrived at, it is highly probable that men, even medical men, would select their life companions rather on the ground of personal attractions, agreeable manners, intelligence, and accomplishments, upon every other ground than that of their potential qualities for becoming the progenitors of giants, heroes, or octogenarians. Impressed by the large share which females bear in contributing to maintain the birth-rate, and constrained perhaps by the title of his principal essay, Dr. Allen has ignored altogether the depressing, restraining, destructive influence which man must exercise upon the number and condition of the fruits of marriage or concubinage, and yet there is reason to suspect that this influence is darker and deadlier than any other. One of the most important and triumphant inferences which may, we think, be drawn from these inquiries, is that Malthus and his followers were wrong in attributing to the supply of food, climate, and other surrounding causes outside the body itself, the law of the increase of population, and not, in main part at least, to the productive powers of the human body, to the

desires and to the capacity for their gratification, to factors which, although acted upon by external agents, such as the quantity of nourishment available, assuredly act independently of its presence, and often in defiance of its absence.

In denominating our author a pamphleteer the designation is not used in an obnoxious sense, but for the purpose of suggesting that he should collect and consolidate the various members of this series upon connate subjects into a systematic work, which, when purged of a few unnecessary amplifications, unavoidable repetition of ideas, even of phrases, would prove a most acceptable acquisition to the moralist as well as to the physician.

Tilden's Chemical Philosophy.¹—Amongst the host of small new books on chemistry, with most of which we could easily dispense, here is one work of very different aim to the common crowd. And we are glad to say that the execution of Dr. Tilden's 'Introduction' is as commendable for accuracy and thoroughness as the aim of the book is high. The volume treats of that aspect of chemical science which borders on physics. It attacks such problems as the constitution of matter; fusion, solution, and crystallization; liquid diffusion and dialysis; evaporation and ebullition; diffusion and dialysis of gases; relations of gas to temperature and pressure; and spectra. All these subjects are treated in the light of modern views in the first section of Dr. Tilden's 'Introduction.' Then follow a few pages of judiciously selected and ingeniously planned problems—problems which, in many cases, have quite a distinctive character of their own. We mean that these questions or exercises are not by any means of the usual commonplace type, but that much care in their construction has been evidently employed. They are not to be worked out without some thought on the part of the student, while they fully illustrate the principles enounced in the text. The same encomium must be passed upon the four other sets of exercises, one set following each of the sections into which the volume is divided; indeed, we regard some of the later and more advanced exercises as the best and most characteristic.

We need not reproduce here the table of contents belonging to this volume; suffice it to say that on such subjects as chemical equations and reactions, chemical attraction, molecular weights, types, isomerism, classification of elements and of compounds, Dr. Tilden's views are in harmony with the best modern theories, are announced with clearness, and presented in an attractive form. Every chapter in the volume contains abundant materials for class demonstrations of an interesting and valuable order. And it is

¹ *Introduction to the Study of Chemical Philosophy*. By W. A. TILDEN, D.Sc. Pp. xvi and 279. London, 1876.

precisely the philosophy of chemistry which runs a great danger of being overlooked in cramming up facts and phenomena for examinations. Dr. Tilden's book will act as a valuable corrective to a prevalent tendency to look at the outside alone of chemistry, and at the same time it will prove what stores of educational material belong to this science.

Kollmyer's *Chemia Coartata*.¹—This is a sort of dictionary boiled down. We hardly know whether the product of the evaporation is amorphous or crystalline; we fear that the former adjective expresses approximately the nature of the material presented to our notice. But the book will be useful to those for whom it was designed—students reading for examinations, persons who want to know something about the new notation and formulæ of chemistry and about the more recent advances of the science. It is not, however, always easy to turn up the desired information in this volume, owing to the want of a table of contents, and it must be confessed that style and accuracy have sometimes been forgotten.

Madden's *Health Resorts*.²—Dr. Madden's book professes to differ from other works of the same class in its superior accuracy, as being the result of personal observation, or, more properly, as containing his impressions of various places which he visited for his health.

It is questionable how far the feelings of an invalid, that is, of any single one, are a fair test of the climate of a station. It is seldom that two invalids have the same opinion of the same place; and therefore to enable a person to form a dispassionate opinion he must learn what, not only a variety of patients think of a particular climate, but what the local physicians and non-invalid visitors think of it, and then balance their views. Making allowance for Dr. Madden's naturally trusting a little too much to his own impressions, his work is fairly satisfactory, although not quite brought up to the present date. Indeed, we are not on the whole sure that the present edition is an improvement on the two original volumes. For instance, in the case of Egypt an account which the first edition gave of the voyage to Alexandria is now omitted, although it would have been useful if revised, while we have more than two pages devoted to a narrative of an ill-managed trip to the Pyramids which the author took sixteen years ago. There is no account of any of the establishments or homes in Cairo or its neighbourhood that lay themselves out for invalids; there is nothing about the canal or Suez, or the air of the desert, yet invalids who are constantly pass-

¹ *Chemia Coartata, or the Key to Modern Chemistry*. By A. H. KOLLMYER, of Montreal. Pp. 111. London, 1876.

² *The Principal Health Resorts of Europe and Africa for the Treatment of Chronic Diseases*. By THOMAS MORE MADDEN, M.D. London, 1876, 8vo, pp. 229.

ing through Egypt would be glad to get more information about them. We find no account of the recently opened sulphur baths of Helivan, some sixteen miles from Cairo, where a hotel has been opened, and where the sulphur baths, which are thermal and resemble those of Aix-les-Bains, may be used while the air of the desert is inhaled, yet an account of all this might have been found in Murray's handbook for Egypt. In like manner the account of Algiers appears to be pretty much the same in this as in the first edition, and the references are chiefly to the older French writers. Although the name of Hammam Melouan is mentioned, no account whatever is given of the place or of its waters. Then, various well-known health resorts are not mentioned at all; there is no account of Ajaccio, of Catania, of Meran, or of Montreux, nor of the southern slopes of the Alps, nor of the many summer stations in Switzerland and other parts of Europe. The notice of St. Remo extends only to a few lines, yet at the present moment it is probably more popular than Mentone, and almost rivals Cannes.

If we now turn to the second portion of the work, that devoted to mineral waters, we here also find it very deficient even as a popular guide, and its information is neither very recent nor very accurate. There is a fair enough preliminary sketch of the classification and of the operation of mineral waters. There is also a chapter on uterine complaints, scarcely, we would suppose, suited for the use of the invalids for whom the book is mainly designed.

Among omissions we observe that the baths of the Black Forest, Ripoldsau, Griesbach, &c., are not once alluded to. We recently observed a pleasant notice of them in a weekly periodical in which regret was expressed that they were not known to the English, while its writer might have found a tolerably full account of them only two years ago in the very journal in which his communication appeared. In another part of Germany a whole group of springs, among which Lippspringe, Pyrmont, and Rehme, are the chief, are omitted, and there is no allusion to the treatment of cases of progressive locomotor ataxia, at present much heard of, at the last of these places. An unnecessarily long account is given of the sources of the purgative waters, which no one ever resorts to, such as Seidlitz, Saidshütz, and Püllna, and their analysis is given at full length; while we have no account of the composition of the very popular Friederichshall and Hunyadi waters.

Or if we turn to France, Dr. Madden has never heard of the new great establishment at Dax. He knows only of "a large roofless structure of considerable antiquity." As a specimen of the way in which some of the smaller watering places are dealt with, we give the two following extracts:

"Above fifteen miles from Montdore near Murol, is the watering place of St. Nectaire, also in a volcanic district. There are several

thermal springs in this locality, the temperature of which varies between 75° and 110°. They are all alkaline, ferruginous, and stimulant. They are principally used in cases of renal and hepatic diseases; in enlargements of the liver or spleen, and are also employed in amenorrhœa, leucorrhœa, and gout. The last of the spas of central France to which I shall allude is Royat, situated eight miles from Clermont. The waters of Royat closely resemble those of Mont Dore, than which they are, however, one third stronger. Royat is in considerable repute with many French physicians in the treatment of scrofula, gout, and rheumatism."

We cannot help thinking all this very vague and inaccurate; who could guess whether there is any accommodation likely to suit the English at St. Nectaire, or that they are now building a large bathing house there? Who could guess that Royat is a particularly lively pleasant little bath, with waters having a very much stronger analogy to those of Ems than to those of Mont Dore, and that Royat is, according to Constantine James, a quarter of an hour, instead of eight miles' distant from Clermont. How too was the arseniated spring of La Bourbole not mentioned in connection with Mont Dore?

It would be easy to point out a variety of oversights and of omissions, but we have only space to allude to the unusual number of typographical errors. Dr. Madden has made various apposite quotations from the classics, but strange to say the misprints of the original edition are perpetuated in the present one. We have *terras longiniquas* for *longinquas*, and *cælumque profundus*. The old quotation of Ausonius praising the climate of Nice is repeated, while it was pointed out long ago that Ausonius never spoke of Nucea as his "natale solum," but of Bordeaux of which he was a native.

On the whole, although Dr. Madden's volume brings together a great deal of useful information, we cannot agree with him in thinking that he has produced a volume at once portable and of superior accuracy.

Ringer's Therapeutics.¹—A book that has acquired so extensive and well-earned reputation as Dr. Ringer's 'Handbook of Therapeutics' has done, needs no commendatory notice as each new edition issues from the press. It would be enough to announce that a new edition has appeared; its known value would secure it purchasers and readers; but some may possibly inquire wherein the latest differs from the former editions. Now, on this matter we have searched for ourselves, comparing several of the most important drugs as described in this last edition with what was written of them

¹ *A Handbook of Therapeutics.* By SYDNEY RINGER, M.D. Fifth edition, 1876.

in the immediately preceding one, and as the result of our investigation can confirm the intimation given in the preface to this fifth edition, that an endeavour has been made "to put the reader in possession of the new facts observed, and to give an account of various important discoveries made since the publication of the fourth edition." As an example the new article on *gelsemium sempervirens* is quoted, but, in our opinion, this is not an example of the first order, as the notice of the drug is but brief.

The number of pages in the work has not increased, but a larger size has been adopted, and the "handbook" character threatens to be extinguished by successive development.

The author acknowledges his indebtedness "for a considerable part of the additional information touching the physiological action of many drugs to the learned and valuable work of Dr. Horatio Wood." He also repeats his thanks to his friend Dr. George Bird, who has assisted him in previous editions; and on this occasion has also to thank a new collaborateur, Dr. Theodore Maxwell, who has prepared for this edition an elaborate analytical index of diseases, the reputed remedies for which are subjoined, with notes respecting their doses and other matters,—a very painstaking and useful production.

The improvements now recounted add to the value of this esteemed work, and sustain its character as the best treatise on therapeutics we possess.

Jones's Medical and Surgical Essays.¹—This is a bulky volume of between eight and nine hundred pages, printed in small type, which testifies to the industry and professional zeal of the author. It conveys the results of observations and researches carried on for twenty years, embracing the period of the American Civil War, 1861—1865; and it is addressed primarily to the medical practitioners in the Southern States. The cases reported have been drawn, to a large extent, from hospital and military service, in which the author has been largely engaged. The present volume is to be followed by two others.

If the Civil War afforded Dr. Jones a wide field of observation, it seems also to have brought upon him many troubles, and as a former medical officer of the Confederate Army, to have deprived his labours of the recognition they deserve. He bewails the literary disadvantages he has laboured under, the absence of personal intercourse with learned men, and his want of access to large public libraries; and he has to deplore the destruction or carrying-off from Richmond, when that city fell into the hands of the troops of the

¹ *Medical and Surgical Memoirs, containing Investigations on the Geographical Distribution, Causes, Relations, and Treatment of Various Diseases.* By JOSEPH JONES, M.D., &c. Vol. i. New Orleans 1876.

United States, of a large portion of his manuscript, the fruit of several years' labour and research.

It is not in our power to present a review of the contents of this large volume, although we consider some of its chapters well worthy careful examination and study; particularly those on the pathology of traumatic tetanus and on cerebro-spinal meningitis.

His first chapter forms an introduction to the study of diseases of the nervous system. It presents an historical review of the progress of knowledge concerning that system, and an analysis of existing opinions, and is chiefly physiological. It indicates a careful and appreciative study of the best authorities, and the impressions made on a well-trained and cultivated mind; still, though an essay of much interest and the fruit of wide reading, and of especial value to those who desire to know the present aspect of nerve physiology and cannot devote the time to search for themselves, it offers no features demanding the special consideration of a reviewer. In the chapter on tetanus we are presented with original observation and experiment, as well as with a critical review of the results arrived at by other investigators. Dr. Jones deals with the natural history of the disease, examines its relations to other convulsive maladies and to climate, and, after detailing a vast number of experiments on animals with various poisons, concludes with a review of the treatment of tetanus, including a tabular analysis of observations made by a great number of physicians and surgeons. Unhappily he cannot point to any definite or special treatment which can be regarded as successful, although his analysis of 420 cases of this terrible malady convinces him that important advances have been made towards efficient treatment.

The chapter on cerebro-spinal meningitis has especial value as a record of experience of the disease as it prevailed in the army of the Confederate States during the civil war. The position occupied by Dr. Jones in that army, and the orders issued in his favour by the Surgeon-General of the time, enabled him to obtain reports from the various surgeons in whose regiments the malady made its appearance. These reports contain notes of the morbid features presented during life, and of the pathological appearances met with after death; and, in many instances, the records given are sufficiently full and exact to afford valuable data for arriving at conclusions respecting this very fatal and not well-understood malady. A considerable space is devoted to the relations of cerebro-spinal meningitis to malarial fever, whilst the pathological anatomy of the two diseases is tabulated and contrasted in opposite columns. The general outcome is, that although many of the symptoms of the former malady resemble those of certain forms of malignant malarial fever, the nature of the lesions and the entire pathology of the disease differ.

The tenth chapter includes "clinical observations on certain diseases of the lymphatic and circulatory systems, and of the liver and kidneys, illustrating the relations of dropsy to various diseases." It is accompanied by details and experiments on osmosis, and on the absorption and action of purgatives.

The next chapter is occupied with the pathology of the different forms of dropsy, and is followed by chapters on the prevalence and fatality of pneumonia in the Confederate Army, on its treatment, and on its relations to malaria. The concluding chapters are devoted to the pathology of mollities ossium, and to the relations of this lesion to fragilitas ossium.

Enough has now been stated to show the character and contents of this volume, and enough, we hope, to indicate their importance and value, particularly to those whose attention is directed towards the elucidation of the pathology of traumatic tetanus and of cerebro-spinal meningitis. We shall look forward with interest to the appearance of the further volumes promised.

Garrod on Gout.¹—It is rather appalling to a student of medicine, wishful for a full and thorough knowledge of his profession, to have placed before him a large octavo volume of nearly 600 pages devoted to one malady and a pathological congener, as representing the outcome of the present state of information on the subject. Happily, however, for him, he can find in several text-books the sum and substance of our knowledge of it in very much shorter compass, and if he turns to this elaborate treatise it is rather as a book of reference he uses it, or as an affair "de luxe."

Moreover, the dimensions of the book are augmented by the large and well-spaced type used, and by the notes of a considerable number of illustrative cases. By these circumstances its perusal is made easier. But, after all, the impression will remain that all we know and need to know about gout and rheumatic gout might be imparted in fewer pages and with less trespass upon the life-space of the reader.

This new edition of this well-known and very valuable treatise on gout contains additional matter by the author, drawn from the experience of several years since the previous edition appeared, and it is also enriched by observations gleaned from the notes of Dr. Charcot, who edited the French edition, and by some drawings derived from the same source. Dr. Garrod, however, adheres to the views he put forward when he first wrote on the subject in 1860, and consequently the contents of the book are not materially altered.

Its comprehensiveness and completeness will continue to render

¹ *A Treatise on Gout and Rheumatic Gout.* By ALFRED B. GARROD, M.D., F.R.S. Third edition, thoroughly revised and enlarged. London, 1876.

it the chief book of reference on gout as at present understood and treated.

Otis on the Transport of Wounded by Railway.¹—An adequate and thoroughly organized system of transport is essential to the efficiency of a modern army in the field. The objects for which this is required are to carry provisions, forage, and military stores, and occasionally reinforcements of troops, to the front; while, to the rear, they are chiefly the removal of sick or wounded, a large percentage of whom require to be carried in the recumbent position. As these services require very different conditions in the carriages appropriated for them, in civil life they are met by having separate vehicles embodying these conditions, which can be provided in numbers sufficient for the ordinary stream of traffic, and this can be conducted so as to prevent one portion of it interfering with another. With an enemy in motion the circumstances are quite different; a large force is collected at a particular point at a short notice, an amount of traffic is directed to this point, exceeding what the roads or railways have been calculated to accommodate, and there is not time to provide the additional facilities required ere the necessity for them, perhaps, has passed away. Special carriages for special services, therefore, can only be introduced to a limited extent, and the exigencies of supplying forage and stores may be so great that even that limited number may be with difficulty brought to the point where they would be of use at the moment they are most urgently required. That these difficulties are real is apparent from the fact that, though the Prussian army, in the war of 1870, had twenty-one hospital trains, fitted with every convenience, capable of removing 3724 recumbent patients, yet only about an eighth part of the sick and wounded transported by railway were conveyed in special hospital trains, and most of those that started for the seat of war during September returned without wounded, or with only slightly wounded and fever patients, from their inability to reach the points where there were more severely wounded to remove.

While admitting the usefulness of regularly fitted hospital trains by railway whenever they can be employed, there has been a strong desire on the part of medical officers and military administrators to provide some arrangement by which the ordinary goods waggons, employed in conveying heavy traffic to the front, might be utilised for the removal of wounded after a great action, when these suddenly accumulate to an extent far exceeding the powers of special trains to cope with, even could they be brought to the spot where they

¹ *A Report on a Plan for Transporting Wounded Soldiers by Railway in Time of War, with descriptions of various methods employed for this purpose on different Occasions.* By GEORGE A. OTIS, Assistant Surgeon, U.S. Army. Washington, 1875, 8vo, pp. 56.

were wanted. The object to be attained is to neutralise the jolting to which waggons without springs, or those with stiff springs adapted for heavy loads are subject, which would be productive, not merely of discomfort, but to something much more akin to torture, to men with severe wounds or gunshot fractures; and this should be effected by simple contrivances, which can be fixed or removed easily and quickly, leaving the waggon efficient for its original purpose. There have been many attempts to attain this object, from a thick layer of straw or small twigs and leaves on the floors of these vehicles, over which the wounded were placed, to springs attached to the floor to carry the ordinary field stretchers, to bars suspended from the sides carrying the stretchers, or to slings suspended from the roof with spiral springs to ease the jolting. The end in view may be attained more or less satisfactorily by any of these, provided the material be on the spot at the time it is wanted, and when springs are employed there be a sufficient number of workmen at hand to fix them in the proper way; but it is one of the difficulties constantly recurring in warfare that a particular appliance is not within reach when it would prove useful; and though a man of resources under such circumstances would cast about for something to effect his object, yet it is well to anticipate such deficiencies, and, in preparing plans to obviate them, to confine the selection to such articles as are simple in themselves and as are likely to be obtained with the army.

In 1873 the Russian Government appointed a committee to arrange the means for removal of wounded, which concluded that service must mainly be effected by the box-cars which constitute nine tenths of Russian railways. This seems to have led Zavodovsky to turn his attention to the mode of suspending the ordinary field stretcher in these cars, and he has devised an extremely simple and inexpensive mode of doing so, by stretching transversely slings of common rope to hooks fixed to the side frame of the waggon just under the roof; from these others hang to carry the stretchers, the lower ends of which are lashed to eyebolts in the floor after the stretchers are placed on them.

It is not possible to make the details clear without drawings, but we may state that the slings can be put up by hand, or unhooked and removed with ease, by any unskilled person, and without the employment of any tool whatever; the hooks and eyebolts remain, as they do not interfere with the use of the waggon for ordinary transport. M. Zavodovsky embodied his views in a small pamphlet, which was transmitted to the American Government, and referred to the Surgeon-General of the Army for his report.

Mr. Otis gives a translation of M. Zavodovsky's pamphlet, and he takes the opportunity of describing the system of transport of sick and wounded by railway, which may almost be said to have

been commenced and brought to its present state of perfection by the medical officers and engineers of the United States during the civil war; for although there have since been many modifications introduced in the Continental hospital trains, suitable to their form of carriage and the conditions in which they are placed, every essential point was anticipated and met in the American arrangements, and in practice their working had attained a high degree of efficiency.

The American passenger cars were particularly well adapted for conversion to hospital purposes. They are much longer than those employed in this country, their doors are in the ends instead of the sides, and the interior forms one longitudinal compartment instead of being subdivided into several by cross-partitions. This permitted of stretchers or bedplaces being arranged along each side in one or more tiers, with a passage in the middle, while the end doors allowed of communication through the whole length of the train. Each car could receive from twenty to thirty recumbent cases, according to the manner in which it was fitted.

Trains were formed of carriages prepared in this manner, and with the requisite accommodation for medical officers and attendants, and for provisions, medical and surgical stores, and cooking, and so were capable of making long journeys with comfort and advantage for the wounded.

Mr. Otis gives details of the different modes adopted in arranging and suspending the stretchers, and illustrates them by woodcuts; he also gives details of the modes adopted on the Continent for similar purposes. For these we must refer our readers to the work itself, and we recommend it to their notice as conveying, in a clear and distinct way, much interesting information.

As to the plan of M. Zavodovsky, which proposes to carry two tiers of stretchers, Mr. Otis fears it would allow of too much oscillation, both longitudinal and lateral, especially in the upper tier, but admits that, without submitting it to the test of experiment, he cannot speak with certainty on this point. We understand, however, that trials of it have been made in this country, which left no doubt as to the gentleness and softness of the motion, and that the longitudinal and lateral oscillations were very moderate when the slings were properly secured to the floor, even under severe tests.

St. Bartholomew's Hospital Reports.¹—This volume opens with a contribution from the pen of Sir Thomas Watson, *in memoriam* of Dr. Peter Mere Latham, in which in his own clear and impressive style is given a brief sketch of the life of his friend, noticing all its most important events, and delineating with artist's

¹ *St. Bartholomew's Hospital Reports.* Edited by JAMES ANDREW, M.D., and THOMAS SMITH, F.R.C.S. Vol. xi, 1875.

pencil the admirable character he has had for the subject of his memoir. After this record of his life Sir Thomas Watson sums up his character in these few but fitting words:—"He was an example of that peculiarly English character, the unobtrusive but high-minded Christian gentleman." For many years past ill health had compelled Dr. Latham to retire from practice and withdraw to the quiet of a peaceful home that he made for himself in Torquay, where the writer of this notice, but a short space since, found him in his eighty-fourth year in full vigour of intellect, enjoying all the calmness of assured religious convictions, and with child-like submission "waiting to be called away." We cannot but be sensible that his life had not been a useless one, that he had won many friends who looked up to him with respect and even veneration. Most truly do we concur with the words of his biographer:—"Ripe in years as he was, and ready in spirit for the solemn change, his death must long be the subject of tender and sacred regret among the nearest and dearest of his surviving family and friends; nor will his memory soon cease to be reverently cherished throughout a much wider circle.

"Multis ille bonis flebilis occidit,
Nulli flebilior quam mihi."

Few in number, now, are those who could have done as strict biographical justice as has here been rendered to the memory of Dr. Peter Latham. Such an "in memoriam" tribute from so accomplished a writer cannot fail to become classical; it is, therefore, with much hesitation we venture to suggest a hint that Sir Thomas Watson has not entirely filled in that part of his sketch which relates to the peculiar endowments and qualities of Dr. Latham as a teacher. Indeed, much more might be recalled by ourselves "in memoriam" of Peter Mere Latham, but our present business is rather to notice the 'Reports.'

First among these is one upon the "causes, diagnosis, pathology, and treatment of missed labour," by Dr. Greenhalgh. This essay expounds a subject which, as its author observes, "is one about which little is known, of which few have had any experience, which has been rarely described, is involved in considerable obscurity, and is, notwithstanding, one of great practical interest." Dr. Greenhalgh has tabulated ten cases of "missed labour," collected from the writings of several authors, to which he has added one of his own.

Dr. Clement Godson follows with an exposition of his practice in the induction of premature labour. This consists in the introduction of graduated sponge tents within the os and cervix uteri. The plan is not only simple, but has the recommendation of being more sure and safe in operation than some others that have been advocated.

Dr. Gee describes a form of angina which, so far as he is aware, has been overlooked by nosologists. Dr. Gee relates cases, and considers that there is an angina which must be called tuberculous, and which forms a part of general tuberculosis. Next, that there is a form of general tuberculosis which may be called anginal, inasmuch as the chief symptoms during life relate to the fauces. Dr. Gee contributes also a note upon the greater prevalence of laryngismus in the earlier than the later half of the year.

Dr. Dyce Duckworth, in this volume, commences a series of "Clinical Contributions to Practical Medicine," which must prove of value, taken in conjunction with the reports from the post-mortem room by Dr. Wickham Legg, containing a store of pathological facts.

An article on the "Treatment of Hæmorrhage from the Posterior Tibial Artery in the Upper Two-thirds of its Course," by Mr. W. H. Cripps, succeeds to these.

Dr. Claye Shaw has recorded an instance of "Uncontrollable Impulse." The patient was an inmate of Leavesden Asylum as an imbecile, but beyond a certain fatuous expression of face there were no signs of imbecility about him. He could read, could get his living out of doors, was active, had good memory, was not epileptic, and had no delusions or hallucinations of any kind; still he confessed to an unconquerable feeling pervading him at times to "do something." Accordingly he would at times smash the windows, always *being very pale* before the act, just like a person in the first stage of an epileptic fit. After the act he became quiet and would confess his sorrow for what he had done, protesting his inability to prevent it. Once he seized a knife and attempted to cut his throat; he afterwards deplored his unfortunate state, saying, "It came on me so strong all at once; it is something in my head." Just before the act he had turned very pale. No other change in his condition was observable.

That there is such a thing as uncontrollable impulse, Dr. Shaw observes, can be verified by facts, and among these facts the preceding case is to be received. The difficulty is, that "whilst an act of destructive impulse in a person already in an asylum, or one who has been formerly under certificate, is allowed for and condoned, a similar act done by a person whose sanity has never been called in question is visited with the extreme penalty of the law. A great step will be gained if the judges can be made to believe in the existence, as a fact, of such a thing as uncontrollable impulse. Whether it has been the cause of violent action in any particular instance requires careful investigation, and this plea should not be too hastily dragged forward as an excuse."

That deeds of violence have been done under the uncontrollable impulse of epileptic disease is a fact that has been fully recognised

by those who have studied mental disorder, although the fact is not clearly recognised by the administrators of the laws. Dr. Shaw expresses himself very strongly, but not too strongly, on this point:—"However cruel it may seem, it would still be the proper course to put every epileptic person of an excitable temperament under restraint; they are liable to explosions of nerve force, and can never be deemed safe. Deaf to all the entreaties of friends and to the man's own requests, a patient who has once shown impulsive insanity of a violent form should never be discharged, for if again he should be hurried into the commission of a violent crime he would suffer for it, or might do so."

It would require no very lengthened search through the records of criminal jurisprudence to find painful illustrations of the truth of these remarks. We may add, however, that not only is epilepsy often the cause of crime, but that, as has been shown in recent instances, the onset of general paralysis has been the cause of sudden breaches of the law by those who had no previous intention whatever of committing the offence.

Dr. Shaw further proceeds to discuss the medico-legal bearings of uncontrollable impulse, and has contributed an essay, which should be studied by psychologists as well as by those engaged in forensic medicine.

Mr. Power publishes a series of cases of injury of the eyes, and Mr. Thomas Jones contributes a short note on the treatment of disease as met with on the Gold Coast, the chief point of which is to draw attention to the fact, that suppurative disease of the liver is very rare in that region, although dysentery is very frequent. Out of about 265 cases of dysentery seen by Mr. Jones not one case of hepatic abscess was present. This statement is opposed to the theory of Dr. George Budd, that dysentery is a frequent cause of abscess of the liver. Again, Mr. Jones states that Phthisis is common among the West Indian soldiery, although in their own country they possess almost an immunity. Ipecacuanha in large doses is considered by the author to be almost a specific in dysentery.

We next meet with records of some cases of congenital dislocation of the hip-joint, by Mr. Howard Marsh; and of the treatment of stone in the bladder of female children, by Mr. W. J. Walsham—all rich in facts and sound reasoning.

An "Essay on the History of Medicine in Ireland," by Mr. Norman Moore, is an interesting contribution to medical literature. Mr. Moore has also related two examples of malformation of the heart.

Dr. Lauder Brunton has supplied an elaborate essay upon irritants and counter-irritants, with remarks on the use of blisters in rheumatism. Irritants, Dr. Brunton observes, cause dilatation of

the vessels of the part to which they are applied, with contraction of the vessels in definite areas of skin corresponding to different sets of internal vessels. The researches and experiments of Dr. Brunton do not support the opinion that blisters are of little or no utility—a view, it may be added, incompatible also with clinical experience.

Mr. Thomas Smith's "Report on the Treatment of Ruptured Perinæum," clear, concise, practical, will be found well suited to supply the wants of the practitioner who may have the misfortune to meet with such cases.

We must be content to enumerate the remaining essays, viz. "On Femoral Aneurism cured by Digital Pressure," by Mr. Morant Baker; "Cases of Disease of the Lungs," Dr. de Haviland Hall; "Erythema Nodosum," by Dr. Hensley; "Idiopathic Tetanus," by Mr. Hastings; "Air in the Cavity of the Pleura," by Mr. Butlin; "Medical Cases," by Mr. Vincent Harris. These, with the "Proceedings of the Abernethian Society" and the "Hospital Statistics," close the volume, respecting the contents of which we may congratulate its editors and readers.

Browne on Use of Ophthalmoscope.¹—This little book is intended to serve as a guide to the student in his early ophthalmoscopic studies, and is intentionally written in a simple manner. It contains as much information as would be useful to a general practitioner who desires to be acquainted with the mode of using the instrument, and the significance of the more common appearances of the fundus.

Mr. Browne commences with a brief account of the optics of the ophthalmoscope, written in an elementary style, adapted for those whose mathematical knowledge may have been neglected. He then passes to the actual use of the mirror, and gives directions for a kind of ophthalmoscopic drill by which the student can attain, step by step, the necessary manual dexterity in the management of the light and the accessory lenses.

Next, the natural appearances of the fundus are described in detail, and Mr. Browne rightly lays great stress on the pains and attention due from the student in order to acquire a trustworthy knowledge of this portion of his subject. The details of the optic disc are described with great clearness, and so arranged that the student can easily acquire the art of examining them one by one, and not fall into the common vicious habit of judging general appearances. In his account of pathological appearances, Mr. Browne

¹ *How to use the Ophthalmoscope; being elementary instructions in Ophthalmoscopy.* Arranged for the use of Students by EDGAR A. BROWNE, Surgeon to the Liverpool Eye and Ear Infirmary, &c. 1 p. 108. 35 Diagrams. London, 1876.

does not follow the ordinary rule of describing the individual diseases, but directs attention to the signs of variation from the normal in each structure. The changes occurring in the optic disc and the retinal vessels are described with accuracy and sufficient fulness.

Especial care is taken to make the student familiar with the changes of the surface level as in the varieties of cupping, and the elevation from intracranial disease or neuritis.

This portion of the book will be found useful to a larger audience than that Mr. Browne professedly addresses, as the recent progress in the diagnosis of nervous diseases has rendered an acquaintance with the facts connected with neuritis and atrophy of the disc essential to the physician who would be level with the knowledge of his time.

The volume is well printed and copiously illustrated by diagrams, and may be safely recommended to all who desire to commence intelligently the ophthalmoscope.

On Medical and Surgical Electricity.¹—The great difficulty of electrical treatment in ordinary practice is the time and trouble it requires of the physician. Apparatus have now been devised both for faradism and galvanism which are easy to work and easy to keep in order, and we disagree, therefore, with Drs. Beard and Rockwell, who assert that a knowledge of electro-physics is a *sine quâ non* for the therapist.

It is necessary, no doubt, that some one should work at that side of electro-physics which bears upon the adaptation of the science to the medical art, and that such inquirers should familiarize the physician with the main rules to guide his work. But it is no more necessary that an electro-therapist should be an electro-physicist than that a physician should be a botanist or that an engineer should be a mathematician. So many demands are now made upon students of medicine that we think it positively disheartening to add to these demands unnecessarily. We think therefore, that treatises on electro-therapeutics may take a smaller compass by so much as relates to physics, and if this be done we shall have more manageable volumes before us than the present. A short chapter would suffice to represent to the reader primary and derivative principles, and any modifications in electric instruments which the author has found to be useful. Drs. Beard and Rockwell are known to be earnest advocates of a method of electrization known as general faradization. This consists in submitting the whole body to the faradic current by placing a copper plate under the feet and passing the other electrode, often the operator's hand, on various parts of

¹ *On Medical and Surgical Electricity.* By Drs. BEARD and ROCKWELL. Second edition. London and New York, 1875.

the body of the patient. Virtues so incredible both in kind and degree are claimed for this method that we are tempted at times to say that it is contrary to experience that miracles should be true, but not contrary to experience that testimony should be false. However, as we have not tried general faradization we have no right to deny that by its means after a few sittings a weakly ailing woman, may become as robust as a milkmaid; the lame may walk, and the eczematous be cleansed. We must admit, however, that the book reads to us too much like the story of wonderful cures one sees appended to the advertisements of electric belts and magnetic hair-brushes, and for this we blame the authors, who in their turn may blame our stupidity. We do not say that they omit all their failures, nor do we assert that they fail to suspect any fallacy in their way of putting things, but we do say that there is a plentiful lack of rigour in their method and of precision in their observations. There may be a good deal or some little in their assertions, but we certainly do not recommend their treatise to young practitioners.

A Text-book of Electricity in Medicine and Surgery.—Dr. Poore's book is the very reverse of that of which we have just spoken, it is as satisfactory as the former is unsatisfactory. Dr. Poore has not electrified the public indiscriminately and written down loose records of wonderful results, but he sees clearly that electricity is no more a panacea than quinine or cold water, and that to avail ourselves fully of its virtues, we must know very precisely the nature of the cases to which we apply it, and the way in which electricity manifests its effects. In a short introduction he gives us all that any practitioner requires to know of the principles of electricity and its applications, and he gives more space to a description of electro-physiology, of the methods of using electricity, and its value in diagnosis. The author then treats his subject medically, under the somewhat unusual heads of electricity as a stimulant, and electricity as a sedative. Such a division is, we think, interesting as an experiment, and useful, so far as it groups a good many things under some convenient features of likeness. We think, however, that a division which is clear enough to please in a small book is not really distinct or thorough enough for a more complete work, seeing how little we really know of the real actions and differences of stimulants and sedatives.

The quality of the book is excellent, for in it the author does not treat his subject as a mere specialist, but wedding his electrical experience with his general medical learning, he rises to higher conceptions of the whole of his subject. This is especially true of the chapter on "Fatigue diseases," a group containing writer's cramp,

¹ *A Textbook of Electricity in Medicine and Surgery.* By GEORGE V. POORE, M.D. London, 1876.

and other pains, tremors, and cramps, in which Dr. Poore does not treat us to a mere record of wonderful cures, but reasoning as a physiologist and electrician, he gives us a very fresh view of these affections, and one which, we think, will in substance be generally accepted. As an electrician more exclusively Dr. Poore seems to have met with a great deal of success in relieving writers' cramp, and this not by any new process, but by a closely accurate adaptation of the old, which reminds us of Duchenne. Treatment by drugs again is by no means forgotten, and the reader thus obtains a clear account of the nature and characters of disease with all the indications for treatment, which these and experience commend to us.

After our perusal of Dr. Poore's volume, we are glad to think we can add it to those of Althaus, Tibbits, Radcliffe, Reynolds, and Walter Smith, in support of our national reputation, in face of Ziemssen's work in Germany, and Duchenne's in France. Like Mr. Walter Smith's little book, this by Dr. Poore is not comprehensive enough to take the place of a standard treatise; but we would urge upon both of these gentlemen to pursue their inquiries in a like acute and patient spirit over the remaining part of the field of electro-therapeutics, and to present us with results which will be of enduring value, and which will ensure the lasting reputation of the authors.

Reproductive Disorders.¹—Who this *Court* physician may be, or what his real object is in the publication of this work, is a little obscure. It certainly is somewhat redolent of the advertisements of those singularly charitable individuals whose sole aim of life seems to be the benefaction of their species, and whose private addresses can, no doubt, be afforded by the proprietors of the newspapers in which their notices occur.

The present work is, it appears, intended for the "professional reader and student," and contains material, the nature of which either is tolerably conversant. It seems somewhat a pity that the "public" is not to be included in this generosity. The author, wonderful and to be envied man, is "wholly independent from a *pecuniary point of view* of his profession," and has "not scrupled to freely discuss delicate topics ordinarily shirked or tabooed by prudish or timorous writers Not many years since," our *Court medico* goes on to state, "the physician would, by noticing them, have endangered his professional reputation and social status, and been regarded as a medical Ishmael. Now a healthier tone of thought prevails."

¹ *Reproductive Disorders; Spermatorrhagia; Exhausted Brain, &c., their native symptoms, pathology, and successful treatment.* By a *Court Physician*. London, 1876.

The contents of the work may be inferred from our notice. The unknown professional Ishmael has done his work fairly, and what he writes is fairly in accordance with the literature of this unsavoury subject.

We sincerely trust that his withering contempt of "advertising charlatans" may not suggest an old allegory of glass houses.

Operative Surgery on the Dead Body.¹—Any work from Mr. T. Smith's pen will be looked upon with respect, and, moreover, we may feel perfectly confident that he would not associate with himself any one whom he could not thoroughly trust to carry out his ideas. On the whole, we are pleased with the volume before us, notwithstanding the plethora of works of this kind which are appearing; just now it is nicely got up, well printed, fairly illustrated, the descriptions are clear, and the arrangement, if not novel, concise and methodical. But we must take some objection to such a title as "operations on the *dead* body." It is not at all likely that the student's "'prentice" hand is to be tried on the living, and we feel convinced that such descriptions of operations as are to be found in works on operative surgery, or surgery generally are equally as valuable as companions to the dead-house.

We are quite aware that students must learn on the subject, but we are far from being firmly convinced that methods and processes described in works like the present are applicable to both dead and living.

As a guide, then, to a student preparing for examination or self-tuition in manual proceedings, the book is all that can be desired; but the learner must be always reminded that he may be led away by his success in the dead-house, and find himself grievously at fault in an actual operation.

In our opinion the interesting feature of the work is the series of ingenious sketches of the collateral circulation set up after ligation of the various arteries.

There are one or two omissions, and we particularly notice that of the operations for cleft palate, which is somewhat remarkable, considering the author's skill in its performance; staphylography can be perfectly well taught on the dead body, and some of the difficulties of placing deep sutures in this trying operation mastered.

How much beyond the "frequent and exact references to the anatomy of the parts concerned" is editorial and how much is not we are unable to tell; but we must take objection to the habit which we notice is rather on the increase, of junior members of hospital staffs "editing" their seniors' works by short foot-notes.

¹ *A Manual of Operative Surgery on the dead body.* By T. SMITH, F.R.C.S., and W. J. WALSHAM, F.R.C.S. London, 1876. Second edition.

As an instance of the information to be gained from such interpolations, we notice the singular sentence to be found as a foot-note to page 61, which we defy any grammarian to account for or any reader to understand.

To the senior student, who has no demonstrator; or to the demonstrator himself; and to the practitioner, who desires to "rub up" his operations, we recommend this work.

Application of Wire Ligature in Compound Fractures of the Jaw.¹—The author advises his method in primary treatment in any case of compound fracture; meaning, by compound fractures, those in which the periosteum and surrounding tissues have been lacerated, and allow some degree of primary displacement. He passes a drill through the bone on both sides of the fracture, at the reflection of the mucous membrane, taking care not to injure the fangs of the teeth; a silver wire is next passed through on the one side from before backward, and then, by means of a peculiar needle, brought from behind forward through the hole on the other side of the fracture. Each free end of the wire is now coiled by means of a key; and the degree of tightness, and therefore of approximation of the fragments, is determined by the tension of coil. The method is very ingenious, and apparently very effective. The work is excellently illustrated.

Outline Drawings for recording the situation and form of Skin Disease.²—These outlines form a handy and ready means of recording cases of skin disease. The figures represent various aspects of the body and limbs. The female form has been preferred to the male, as requiring the more comprehensive outline of the two, but they are designed to serve for either sex. The idea is precisely similar to that of the outline sketches in general use for recording lung and heart disease. The figures are clearly and ingeniously drawn, and must be of great value to the practitioner and student.

Hygiène de la Voix.³—This is a useful and well got up little work, chiefly noticeable for a somewhat novel additional form of advertisement, namely, a reproduction on the outside of the book of a plate within, probably a portrait of the author examining a patient's larynx; the original engraving of which is to be seen,

¹ *Cases in Surgery; illustrative of a new method of applying the wire ligature in compound fracture of the lower jaw.* By H. O. THOMAS. Second edition. Liverpool.

² *Outline drawings of the figure for recording the situation and form of cases of skin disease, and for noting their progressive changes under treatment.* By BALMANNO SQUIRE, M.B. London.

³ *Hygiène de la Voix, parlée ou Chantée suivie du formulaire pour le traitement des affections de la Voix.* Par P. MANDL. Paris, 1876. Pp. 304.

showing all the details of his consulting room, at p. 33. As the author has produced a really classical work on this subject, in his '*Traité Pratique des Maladies du Larynx et du Pharynx*,' we are somewhat at a loss to know any ulterior object of the present work beyond mere advertisement.

Hay-Fever, or Summer Catarrh.¹—Hay-fever, which undoubtedly prevails at certain seasons in Great Britain, and has received a due share of notice in medical works, appears to be far more prevalent in the United States than in our own country or in Europe generally. Indeed, we learn from the pages of the present little work that in Bethlehem, New Hampshire, there exists a United States Hay-Fever Association, composed of sufferers from this disease who are brought from different sections of the States to the White Mountain region, where they enjoy an immunity from their malady. The objects of this association, which is composed of lay as well as medical members, and of both sexes, are the communication of mutual benefit and the distribution of useful information to all sufferers from hay-fever, wherever they may be found, and the proceedings are regulated by an organised Board consisting of President, Vice-Presidents, Treasurer, Secretaries, and other officials. The author states in his Preface that he expects his work to be read chiefly by the laity, and among the sufferers from the disease which it describes, and he therefore excuses himself for avoiding purely technical words and phrases. Nevertheless, the work contains a considerable amount of very useful information both of a statistical and therapeutical character. Dr. Beard's theory of the nature of hay-fever is that it is a nervous affection, but he explains (perhaps rather unnecessarily in a medical work) that he does not mean that it is an imaginary one; and his theory does not dispense with the agency of certain irritating causes, as heat, dust, pollen, &c. His opinion, briefly stated, is that hay-fever is a complex resultant of a nervous system especially sensitive to irritation, and acted upon by the enervating influence of heat and by any one or several of a large number of vegetable and other irritants. The most efficacious treatment, after the failure of ordinary medication, consists in the removal of the patient from the locality where he suffers into one where the disease does not prevail; and, as far as the United States are concerned, it appears that certain mountainous regions, and among them Bethlehem, are the best sanatoria for the purpose.

¹ *Hay-Fever or Summer Catarrh; its Nature and Treatment, based on Original Researches and Observations, and containing Statistics and Details of Several Hundred Cases.* By GEORGE M. BEARD, A.M., M.D. Pp. 266. London and New York, 1876.

On Tracheotomy in Diseases of the Larynx and Trachea.¹—The principal merit of this little work consists in the fact that the author writes chiefly from his own experience, which appears to have been pretty considerable in the operation he describes. Thus the various urgent circumstances which may necessitate tracheotomy are very fairly detailed, the steps of the process are clearly indicated, the various kinds of apparatus necessary for the operation are fully described and figured, and the dangers likely to be incurred during and after its performance are carefully brought into view. Among the dangers and difficulties sometimes presenting themselves is one peculiarity which occurred among Mr. Thornton's earliest cases, consisting of a stricture of the trachea below the incision, and in which surgical interference was consequently worse than useless. The entrance of air into the veins is also mentioned as a possible source of danger, but Mr. Thornton gives no case of this kind from his own experience.

The best part of the book is that in which the different kinds of instruments and apparatus necessary for tracheotomy are described; for although, as the author observes, the only instruments absolutely necessary for the uncomplicated operation are a scalpel, a pair of forceps, and a tracheotomy tube, yet, as many complications and accidents may present themselves in certain instances, it is necessary to provide against all contingencies by a sufficient armamentarium of accessory appliances, which may be useful in every emergency. We do not, therefore, blame Mr. Thornton, but quite the reverse, for his minute details as to the selection and employment of all the necessary aids, such as hooks, retractors, pillows, sponges, tapes, &c.; and his description of the different tracheotomy tubes, with their illustrative woodcuts, appears to us to be accurate and complete.

In other respects, however, Mr. Thornton's book is not so satisfactory. The section on the Anatomy of the Trachea is meagre in the extreme, and the pathological history of the various conditions which may necessitate tracheotomy is very unsatisfactory. Croup and diphtheria are considered together, but the author gives no opinion as to the question whether they are different or identical; and although he states that "in these diseases tracheotomy is far less satisfactory than in any other morbid condition in which this operation is admissible, except perhaps, when it is performed for scalds in the larynx," yet he gives no reasons for this statement. When he announces, too, that when the false membrane is in the larynx, but has not passed down into the bronchi, the operation may be

¹ *On Tracheotomy, especially in relation to diseases of the Larynx and Trachea.* By W. PUGIN THORNTON, Surgeon to the Hospital for Diseases of the Throat. Pp. 70. London, 1876.

resorted to with every prospect of recovery, he is perhaps indulging in a more favourable prognosis than is warranted by past experience, and although he draws a distinction between the degree of danger involved in the presence or absence of false membrane in the large and small bronchi, he gives no rules by which such diagnostic facts may be ascertained.

Syphilitic laryngitis, one of the most common affections requiring the operation of tracheotomy, is perhaps one in which the proceeding is most likely to be successful, and out of thirty-eight cases operated upon by Mr. Thornton, sixteen were for this disease. Chronic laryngitis, by which term Mr. Thornton designates an affection unconnected either with cancer, syphilis, or phthisis, but leading to ulceration of the glottis and necrosis of the cartilages, and speedy death, is described as being the next in order to syphilitic laryngitis as requiring tracheotomy; but we may observe that in laryngeal phthisis and carcinoma laryngis, also enumerated, the value of the operation must surely be more than doubtful.

The general literary style of Mr. Thornton's work leaves very much to be desired.

Treatment of Syphilis.¹—We cannot commend either the style or the substance of this work, which treats of a very difficult and complex subject in a superficial and unscientific manner. To explain the treatment of a disease presenting so many and various manifestations as syphilis does, without any attempt at a discrimination of the importance and meaning of the symptoms other than is implied in their bare enumeration, appears to us to be utterly useless. This, however, is what Mr. Milton attempts. Single symptoms are mentioned, often with no attempt at description, and scarcely ever with any reference to pathology, and after each is given the treatment recommended.

The author says in the preface, "It will probably be objected that enough has not been said about the labours of others in this department. But the nature of the work did not admit of this. The book is a digest from practice, not a compilation; and consequently no references have been made except when they appeared absolutely necessary." Yet a considerable portion of the book is devoted to a description of methods of treatment which are mentioned, apparently, only for the purpose of condemning them, or of showing their inferiority by comparison with the treatment recommended by Mr. Milton.

We give an example of the author's style and method (p. 108). "Disease of the ear.—Secondary syphilis in this part requires no

¹ *The Treatment of Syphilis.* By J. L. MILTON, Senior Surgeon to St. John's Hospital for Diseases of the Skin.

particular treatment that I am aware of. Should any local means be thought necessary, glycerine may be dropped into the meatus by means of a camel's hair pencil, and retained by a plug of cotton. Syringing with some weak disinfecting fluid may be requisite, and condylomata may demand the use of some escharotic."

Mr. Milton recommends cauterization of all primary syphilitic sores, and says, "I affirm, without any hesitation, that my own practice has never yet yielded a single case of infection where I have cauterized properly within a week after connection; and I have treated a pretty large number of men who had had intercourse under the most suspicious circumstances."

For the induration remaining after a hard chancre the author advises the application of iodine and other vesicants, which we cannot conceive can do anything but harm; but perhaps these applications are used for the same reason that mercury is recommended on the previous page, "Not from any distrust of the powers of the constitution, . . . but because there are cases where the surgeon must use it with the view of getting the hardness absorbed, or else lose his patient, who does not always appreciate theory, and wishes to be cured a little faster."

Various plans of treating bubo are described (amongst them "the worst mode of treatment ever yet imagined"), for the purpose of condemnation. That which the author thinks is most likely to avert suppuration is the internal administration of tatar emetic. It is difficult to imagine how this drug can influence the suppuration of glands into which a morbid poison has been absorbed; Mr. Milton, however, says: "the surgeon who prescribes it does all that art can do."

After giving a list of the symptoms of constitutional syphilis, Mr. Milton discusses the question whether or not mercury is needful for their cure. He considers that a comparison of the mercurial with what he calls "the simple treatment" (which includes iodide of potassium) shows the balance to be in favour of mercury. But then, as he properly points out, the mercury must be carefully and judiciously administered.

After describing divers modes of treatment which he does *not* recommend, the author gives an account of that which he himself advises, which consists of—"1. Preliminary or preparatory means. 2. Of a course of iodide of potassium, accompanied or not by perchloride of mercury. 3. Of mercurial baths. 4. Of treatment somewhat like the Zittman."

We are not surprised that Mr. Milton thinks his treatment "better than any he has seen used or has read of," for he says, "So far as I know, every case treated in this way has done well, and there have been no relapses." This is a measure of success

that we need hardly say has not fallen to the lot of many surgeons, and it might be a matter of interest to ascertain how far Mr. Milton does know, but that the character of the book is not such as to induce us to spend any more pains upon it.

Treatment of Uterine Versions and Flexions.¹—Dr. Cutter tells us “this is not intended for a piece of fine writing. It is simply a contribution from one who believes it the duty of every one to strive to leave his profession better than he found it.”

That Dr. Thomas, of New York, in his admirable work on diseases of women, not only speaks favourable of Dr. Cutter’s pessaries, but also figures them, is no mean praise. The author of the present work asserts that they have relieved large numbers of suffering women, and this makes him anxious to introduce the pessaries to a larger acquaintance in the profession. So far as we have had an opportunity of judging, Dr. Cutter’s pessaries are little known to the profession in this country. It may be well therefore to briefly describe them.

Two forms of retroversion pessary are submitted—the loop and the T.

The *loop pessary*, includes a belt of inelastic webbing thirty-six inches long, and one and one-fourth of an inch wide, to go squarely around the waist; and a suspensary cord of india-rubber tubing, ten inches long, and one-fourth of an inch in diameter, attached to the middle of the belt by a loop and cord. This is designed to run through the natal (*sic*) furrow (between the buttocks), and is also attached to the perineal extremity of the pessary. It ensures an elastic support, and the furrow prevents lateral motion. The pessary is a cylinder of hard rubber, about one-fourth of an inch in diameter, curved into a hook one and one-fourth of an inch to two and one-half inches in diameter, intended to go around and entirely clear the perinæum. The pessary then curves backward and divides into two branches, curved to correspond with the vaginal curve and terminate by uniting together into a loop. This loop is turned backwards so as to fit into the post-utero vaginal *cul-de-sac*, and allow the cervix of the womb to project into the fenestra made by the loop.

In the middle of the hook there is a joint, so that it may be turned out of the way during defæcation, and (what is of very great importance) allow of a support during this act, where the bearing down efforts have a direct tendency to produce retroversion again,

¹ *A Contribution to the Treatment of Uterine Versions and Flexions.* By EPHRAIM CUTTER, A.M., M.D. Second edition, entirely rewritten, pp. 216. Boston, 1876.

and will certainly cause it, if not prevented, in most instances. The loop is engaged in the post-uterine vaginal *cul-de-sac*, and then holds the uterus in its place; extends the vagina in its long diameter without unduly distending the transverse vaginal fibres, or preventing the normal tonic contraction of these very fibres, which is a most important factor in a healthy vagina. It is not intended to have the instrument take any bearing upon the lateral vaginal walls, for the same reason that it is not common sense to distend still more the already abnormally distended and relaxed walls. It is intended that the instrument should be removed every night and replaced every morning by the patient herself.

The T pessary is like the loop pessary except in the vaginal portion. Instead of dividing into two branches, the single cylinder is continued upwards, with an antero-posterior curve, till it reaches the post vaginal *cul-de-sac*; here it is terminated by a bar like that of the letter T, hence its name. It is lighter and simpler, but more difficult of introduction.

The author resorts to, what he terms, *vaginometry* in order to determine the distance from the perinæum to the posterior utero-vaginal *cul-de-sac*. This he does by means of the vaginal sound, and an instrument specially designed for the purpose, termed a *vaginometer*, a combination of two vaginal sounds, handled and jointed together at the proximal ends. Near the joint, a graduated scale gives in inches and fractions the distance between the bars.

The advantages claimed for these pessaries are:

1. A posterior suspension from a point of support, over the stablest part of the body, viz. the sacrum.
2. Single suspension, so that the deviation of displacement can only be downwards, the natural and desirable way.
3. Elasticity, which imitates the normal tone and rebound of the normally extended and transversely contracted vagina.
4. A perfect fit around the perinæum, through the vulva, along the longitudinal axis of the vagina, and into the post-utero vaginal *cul-de-sac*, maintaining the normalities of the long vaginal axis, and allowing and inciting that of the transverse muscular fibres.
5. Unirritating material.
6. Selection and application by mensuration.
7. A contrivance to hold the uterus *in situ* during defæcation.
8. It is within the patient's control.

For *Retroflexion*, the author adapts an intra-uterine stem to the T pessary, and introduces it in place of the sound. The disc is made movable upon the cylinder, a device the writer tells us which has saved weeks of effort, and which he regards as one of his best inventions—and the most perfect of the set.

Retroflexion, he considers must be dealt with from a mechanical standpoint, like a contracted joint, or a club foot, or strabismus.

For anteversion he relies upon the retroversion loop, or T pessaries, modified so as to have the loop bent forward, or the bar of the T reversed, so that its concavity looks backward.

In cases of antelexion, if the cutter stem is difficult to bear, he has found it advantageous to employ an ordinary stem; then introducing a *bracket* pessary to hold the stem in place without the rigid hold of the first named stem.

This bracket pessary is like the ordinary anteversion loop, except that a rudely triangular piece of hard rubber is articulated on to the narrow end of the fenestra in such a manner as that, when placed at a right angle to the pessary, it strikes a projection and remains fixed. It then supports the disc of the stem.

For *Prolapsus* the author has invented a ring pessary, which is a modification of the T pessary, the bar being continued into a complete circle, which surrounds the lower end of the cervix. If this latter be very sensitive and heavy a cup in place of a ring may be substituted, the cup being merely an expansion downwards and inwards of the under surface of the ring pessary. In this manner the uterus is supported, and the dilated and loosened vagina is allowed to contract, and thus rendered more liable to return to its normal condition of columnar support.

He does not approve of Hodges' pessary.

One evident advantage of these pessaries is that "they take no bearing upon the lateral vaginal walls, and thus do not serve to distend still more the already abnormally distended and relaxed walls." By thus artificially supporting the uterus and converting the vagina into a more or less stretched or rigid tube, atrophy of the muscular tissue is liable to ensue, and if the pessary be worn for any length of time, after the uterus has recovered itself, the condition of the vagina will be more flaccid and less capable of supporting the uterus than before. This will apply also to the ligaments. For temporary use, as a mere scaffolding, so to speak, Cutter's pessaries have proved of much service, but their employment, like all other forms of support need careful regulation and supervision.

An objection may be urged that the pessaries are difficult of self adjustment. This will not be found to occur in practice. Dr. Thomas states that out of several hundred cases in which he has used them, he has never found an instance of failure in this respect.

Patients learn to apply their own trusses in cases of hernia, and with a moderate amount of intelligence, after they have been shown a few times how to introduce the pessary no difficulty is experienced.

A more serious objection is that the cross bar of the T, where much pressure is exerted by the displaced uterus, often endangers the entirety of the tissues. This may be overcome, as Dr. Thomas has done, by affixing to the bar bulbs of different sizes, not only to prevent cutting of the vagina, but to place behind the displaced fundus a mass which will make it fall forwards by *displacement*, and not by pressure.

It would have been well that the author limited his communication to the profession to a description of his instrument, with the indications for its use. The remaining portion of the work is occupied by matter that might well have been omitted and savours somewhat of the sensational. The author seems to have thought it a favourable opportunity for airing some of his fanciful conceits, and had we stumbled upon the latter portion of his work without consulting the earlier we might well imagine we were perusing some quack production intended to frighten nervous females into a belief that they were inevitably suffering from some version or flexion of their womb, and had better apply at once to Dr. Cutter for one of his pessaries.

In speaking of the prevalence of version and flexion he states that "any woman, old or young, before or after the menopause may be suspected of suffering from uterine deviation"—who complains of pretty well every symptom that females suffer from—ranging from leucorrhœa, anomalies of vision without physical cause up to impending insanity. "The subjective symptoms are chiefly neurotic. They are often *mimotic*"—whatever that may mean here.

In speaking of examining patients he says: "If the vaginal secretions are copious, they will suffice sometimes for lubrication" (p. 153.) Could any suggestion be more disgusting! One of the chief objects of anointing the examining digit is to protect the examiner, not merely to facilitate examination.

"Flour eating is causatively related to prevalent uterine disease." "In the treatment of uterine tips and turns, a diet of animal food should predominate." He even goes the length of suggesting that fibroids of the uterus may be diminished by resorting to a mixed animal diet in place of starchy food. His mechanical turn of mind leads him to suppose that "if there were any way whereby a still weaker spot than the womb could be secured without injury to the general system," then when the system, as we say, "took cold," the shock of the "cold" would descend upon the spot and not touch the weakened womb." Peculiarities of style are not wanting, such as the uterus being vexed or *riled up* by the inflammatory process (p. 49), "It does not *orient* the treatment" (p. 204), and many other similar expressions.

Regarded as a contribution to the mechanical treatment of uterine flexions and versions, the work is one of some merit, and the author deserves credit for having suggested an instrument that often affords relief when other means have failed.

Original Communications.

I.—Life—Vitalists and Physicists, Teleology. By ROBERT GARNER, F.R.C.S. Eng., F.L.S., &c.

THE great and obvious difference in characteristics between every inorganic body, even the most specialised as a crystal, and the most simple organic being, leads, as it appears to most people, to the natural conclusion that they belong to two very different classes of objects, governed by forces very different from each other, or rather that the latter owe their endowments to a superadded one, though, indeed, they continue influenced to a certain extent by the purely physical forces, to which all matter whatever is more or less subject. We can imagine mere physical force, as molecular attraction, to form a crystal, but it appears incongruous, at least to many of us, to suppose that any physical force or forces can build up an infusorial, a polype, or a lichen. This last act we, for our part, refer to what appears as much entitled to a name as attraction or any other force—vitality or vital force; the action of which is indicated in such a way that there appears to be a wide difference between it and the action of such force as is seen to work in inorganic bodies. We can imagine bodies, or a world of bodies, not endowed with vital force or action, but cannot conceive them uninfluenced by the ordinary physical.

Though we cannot know what this force or endowment absolutely is, we can observe the peculiarities of its operation, and consequently, though essentially unknown, it is in its characteristics just as discoverable and as appreciable as other forces; it has, indeed, been said to be unappreciable to thought, whilst it appears to be the source of thought itself. If there is ever any difficulty in drawing the line of separation between the two kinds of objects living and non-living, it is only, in some cases, from their extreme minuteness. We may not always distinguish a monad from a molecule, or a bacterium from a fibre of dead matter, but commonly such a difficulty does not exist—there is no borderland between living and non-living objects. There is,

however, a difficulty when we endeavour to draw the distinction between animal and vegetable life in its lowest forms. Hence the division into organic and inorganic bodies is more definite than that into animal, vegetable, and mineral.

If we were asked what are the characteristics of living bodies, an answer might be given as follows. All organisms have a susceptibility as regards their environment, due to the necessity of their interstitial growth—a differentiation of structure, likewise, as the environment becomes more complicated, (though, at the same time, this is not to admit that this complication can *per se* produce this differentiation, and one would even prefer to go back to an *archeus* or *vis insita* instead)—motility or locomotion, especially in animals—a fixed duration of growth and life—a relation both to the past and the future, in origin and in the function of continuing their kind. Essentials for organisation are, 1st, that the matter of which organisms consist should be plastic, neither of extreme hardness nor too fluid; the enamel of the teeth, the shell of molluscs, or the silicated cuticle of grasses is extremely hard, but the living state is ceased in them; the blood, considered by John Hunter to have life, though fluid, is imperfectly so, and prone to solidify; 2nd, there is a limitation of elements, chemically speaking, though this limitation by no means implies that the secondary or proximate constituents are not of a very composite nature;¹ 3rd, the presence of water as a constituent; 4th, a capability of coagulation and solidification and their reverse.

The existence of a crystal is not endangered by the abstraction of a portion, and the portion broken off has the same qualities as the crystal from which it is taken, the same constituents, and no limitation as to duration; but a particle from the organic individual does not, except in the lower forms, or potentially in some cases, present the whole qualities, physical, chemical, and vital, which the individual organic body does. Attraction, molecular or chemical, with some such phenomenon as polarization, appears, as already observed, sufficiently to explain the formation and conservation of organic bodies, though crystallisation is one of the most curious of the effects of these simple causes; not so, as we take it, of organic objects. In the latter, cell-formation and cell-growth predominate; in the former, homogeneity and simple molecular attraction, with its results just mentioned.²

¹ It has been said that the gulf between the organic and inorganic worlds has been bridged by the formation in the laboratory of certain of the proximate constituents of the former. The question is, what are the *true* constituents, and what *excrementitious* matters?

² We know of nothing in the inorganic world so like the effects of organization

In the case of organic bodies it becomes presumable, even from the excentric nature of the force which presides over their formation, a force as much entitled to the name *development* or *evolution* as any other formula which claims it, that in the main there will be an increasing perfection in organic existence, individually, collectively, or systematically, and through time; a fact obvious in embryology, in the anatomy and morphology of animals, and as far as we know in geological life-history; but it is an error to make this increasing perfection to consist in a conecatenated or linear ascent through all orders or groups, rather than an arborescent one with frequent ramifications.

At the present day there are several physiological and philosophical questions connected with this subject, which, though in past times debated and afterwards forgotten or neglected, are afresh exciting interest, and this interest of a very absorbing nature. Such questions are the origin of life on the earth,—though any distinct origin is ignored by the disciples of evolution—the nature, or at least the relations of the vital force, and the cause or causes producing the increasing complication and variations in living beings. The interest of such questions becomes magnified when we find that their discussion may require us to renounce or modify our earliest impressions, or to doubt what we have previously considered as fair conclusions of our understandings.

With respect to the first point of those enumerated—the origin of life on the earth—whether it appears *de novo* and constantly, as regards its lowest forms (heterogenesis), or is only continued through the germs of preceding organisms (epigenesis), perhaps the less said at the present time the better, as the question is, we trust, as far as it can be, fairly *sub judice*.¹ To the present time the first doctrine has, we think, had the approval of many, and moreover, one can hardly understand how believers in evolution, to its full extent, can dispute it; for surely vital germs cannot be required where so little has to be accomplished, the objects so simple and minute, the agencies in other directions so efficient. We will only say, once for all, that if it be necessary to admit either heterogenesis, or the appearance of higher beings

as the dendritic infiltrations sometimes seen between blocks of mountain limestone, putting on the exact similitude of fossil algæ or fuci; but they can be well imitated by art. Professor Tyndall instances the snow-star or ice-flower, the lead tree, and the crystalline structure of cast iron as approaches to what is seen in organisation—phenomena which apparently are the concomitants of what is most antagonistic to organization—extreme cold, strong chemical action, and intense heat.

¹ The writer has been inclined to admit heterogenesis. His doubts of it arise as much from the fact that the minute organisms in question are not found in the morbid fluids of naturally closed sacs, as the ovarian cysts or the tunica vaginalis, as from any laboratory experiments.

on the earth by what has been called natural law, in contradistinction to special interference, it does not appear that there is anything in our professed historical cosmogeny to contravene such views.

Granting, what is commonly considered as proved by embryology, by the growth of the individual, by the supposed greater simplicity of organisms as we go back in geological time, or the reverse, their increasing complications as they succeed each other, that living beings in some way or other undergo a development from more simple incipient forms, we are again arrested, in the first place, with this question—What is the peculiarity of the matter or plasma of these forms on which depends the phenomena called vital, and which we, naturally at any rate, attribute to some intrinsic endowment quite different from what we notice in inanimate objects? We have already confessed that it is difficult or impossible to define this endowment in its supposed real nature, but appropriate to it the name life, vital force, or some such term. But we are told, with great force of language, that to speak thus is but a cloak for our ignorance—that an indwelling tendency to develop, a constructive force, or an ordained becoming of living things, is not a *vera causa*, or assimilable to known causes,¹ and that we must renounce such views, though, in our turn, we may be equally little satisfied with the reasoning which professes to explain the phenomena which we witness by chemico-mechanical and molecular considerations. There are, say the Evolutionists, certain aptitudes of this kind causing matter to evince life; such, for example, are the great contrasts in the primary elements of living tissues as regards molecular mobility, extremely great in those that are gaseous, and the reverse in carbon, and hence there arises in their compounds facility of rearrangement and of development; also there are contrasts as regards chemical activity, oxygen only having an excess of it, whilst the other elements are inert:—unlike units, in other words, are more easily segregated than such as are like. The secondary binary organic elements have less molecular mobility than the above gaseous ones, but more than the corresponding inorganic ones, and chemically speaking, are comparatively unstable, and commonly not very active, but water and carbonic acid are exceptional in some points, and the latter antithetical in relation to organisms—water difficult to decompose, but transformed into vapour at moderate temperatures; carbonic acid greedily seized by water, and constituting the main supply of nutriment to vegetables by its decomposition, whilst, on the contrary, in its formation animals get rid of

¹ Does the *unique* nature of the cause, if we may use such a term, prove that it is not a true cause?

their used-up or noxious materials. As the elements combine into more compound or tertiary proximate constituents their atomic units have greater weight, and consequently the compounds themselves have less mobility; they are mechanically fixed, but chemically unstable, though with no active tendencies: *isomerism* or *polymerism*, however, is often manifest in them, constituting a mode by which redistribution of matter and consequently the alteration of quality is shown; *allotropism* evincing the same effect in the simple elements, carbon, oxygen, phosphorus and sulphur. The most compound or quaternary constituents are non-mobile mechanically speaking, and chemically non-static; they are of high atomic complexity, without polarity, and hence non-crystallisable. The colloid form with its peculiar *energia* can be put on by these compound constituents; it gives plasticity and the power of transmitting fluids and the more active products of decomposition, even through membranes, by *dialysis*. The small atoms of the simpler elements are a requirement, or there could be no organic change; whilst the complex or large atomic units of the compound proximate principles are at the root of the fixity of the organic tissues. The nitrogen is an element of decomposition directly or by catalysis. The quaternary constituents are often only partially soluble in water, and are inert, except when colloid: their large atomic units are mechanically weak, and small forces may break them up; organic atoms are also susceptible of new arrangements from such agencies as the vibrations of heat, light, and electricity. Moreover, whatever deficiency the atoms or units may evince, as constituents of living plasm, is eked out by what is termed "physiological polarity," something not very different one might suppose from what others term vitality.¹ Osmosis and capillary attraction are modes too through which the environments may influence the organism.

The above is, we fear, but an imperfect analysis of the first chapter of the 'Principles of Biology' a work advocating evolution (*in toto*) in a most thoughtful manner, and it must be confessed, at least by me, difficult to controvert, however unsatisfactory it may be. No doubt the properties of organic matter,

¹ "We must infer that a plant or animal, of any species, is made up of special units, in all of which there dwells the intrinsic aptitude to crystallise in a particular way." "In the one case as in the other" (the restoration of the lobster's exuviated claw, and the budding of a portion of the Begonia leaf) "the vitalised molecules composing the tissues, show their proclivity towards a particular arrangement; and whether such proclivity is exhibited in reproducing the entire form, or in completing it, when rendered imperfect, matters not." Spencer, 'Principles of Biology,' 1864. Such units or molecules do not seem to differ much from the "manufactured articles" of Sir J. Herschel or the vitalised "monads" of Leibnitz.

mechanical and chemical, are adapted to the origin, continuation, and action of life, but we are called upon to admit more, or rather, in one sense, to be content with less; we must look to the exterior and its incident forces, and to the organism, or rather to the chemico-physical properties of the constituents of which it is composed, and their respondent and equilibrating actions, and see in these the sum total of evolution and life, and this according to the known laws of physical and chemical force, with which, if we use the term vital force, it must be correlated, being but a modification say of heat, and into it very convertible, as both are into motion. Indeed, we grant that the effect of heat on organic bodies is great and intimate, as for instance, in the germination of seeds, the growth of plants, the development of the egg, and on the functions of the animal frame generally. As remarkable is the effect of oxygen or that of light on plants. But we must not confound collaterals with the cause. Vitality, though intimately connected with heat, shows a distinct and remarkable power of controlling and limiting it to a range of a very few degrees; neither can vitality be transferred to another individual, except by genesis of some kind, being, unlike heat, light, or electricity, non-diffusible, and even limitable by a fine membrane. The physicists too despair of correlating nerve-force with the others, and make but clumsy attempts to explain either it, or muscular contraction; attributing the former to the generation of a facile tract for impressions through the general cellular tissue, and the latter to the subtle and rapid chemical or isomeric action of the unstable muscle. There is, say the physicists, nothing inherent in organisms but an overruling exterior, and their response; nothing to subordinate the whole, unless it is the effect which the increase of one function necessarily brings about in others. Perhaps, however, it remains to be proved that vital function is not originated from within, and also that vital and physical manifestations strictly correspond.

We are required by those who may be termed physicists, in contradistinction to the vitalists, to receive and to rest satisfied with certain definitions of life, which may be said to apply more particularly to life in action or rather to the actions or functions of life—the *totality of functions which resist death—successive phenomena in organisms limited to a period—a continuous movement of composition and decomposition—a series of definite and successive changes in individuals.*¹ But the vitalists infer that life precedes organisation, as is seen in the Monera, in the impregnated egg, or vegetable embryo, and that it may be potential though dormant. It is an endowment by which matter of a suitable quality

¹ According to Mr. Spencer, *the continuous adjustments of internal relations to external ditto*. This seems adaptation rather than life.

attains and maintains individuality for a certain period, and is no longer amenable to the uncontrolled influence of physical or chemical force, but manifests phenomena of a totally different character.

But, further, when the above points are settled, one way or the other, to the satisfaction of our minds, we are brought face to face with other questions which may have still greater interest for us. Whence arise the mechanisms and adjustments which we see in organised beings, and which are by no one more admirably displayed than by the author of the 'Origin of Species'? We are now required to give up those teleological views of intelligent design which may have delighted our minds; and are scarcely allowed to adopt the view, that although all things are governed by unchangeable laws, yet such laws imply a primary intelligence, able, at least, in the language of the Reign of Law,¹ to modify one force by the operation of another. We are referred to an extraneous and mechanical cause, the direct action on molecular matter of the environment, as producing the appearance, and responsive growth, and the differentiation of plants and of animals. With this so-called *law of direct action* we have also one of *indirect action*, tantamount to natural selection, the *direct equilibration* of the organism with the exterior, and its increasing complexity as the exterior becomes varied constitute evolution. But the use of this term appears to be liable to many of the objections which are brought against the use of the term vital force, and indeed we are told that its inscrutable cause lies in the region of the unknowable. With respect to the direct and immediate effect of the exterior as taught by Dr. E. Darwin, Lamarck, and Mr. Spencer, we cannot attribute so great an influence to it *per se*. Can light generate the eye, or sound the ear, the last perhaps as wondrous as the first?

Natural selection is no doubt a true agent in the perpetuation of varieties in plants and animals, but it is not a theory of life, and it is powerless even in its own province without a tendency to vary; on what depends the tendency? Mr. Darwin does not undertake to explain the cause of it; the evolutionist derives it from ancestral proclivities, often of very remote origin. Here we are led to wonder that the *primum mobile* from whence springs that constantly increasing perfection which it is the problem to explain, should be derived from characteristics lower in the genealogical tree, and consequently of a less advanced nature. Evolution, however, in addition, admits that there may be some change in organisms from

¹ See 'Good Words,' Duke of Argyll, 1865.

functional development brought about in the system in direct response to the action of the exterior, and of necessity influencing the structure and functions of other parts *en liaison*, as alluded to above.

But to our minds side by side with the development, or as Owen terms it the derivation of animals, one from another, *purpose* seems evident; and in fact the study of such final cause, instead of repressing research, as it has been said to do, has often been the guide to anatomical discovery. If we are driven from this, some form of Pantheism seems only to remain for us, deifying the natural powers themselves, rather than adopting an exclusively materialistic or molecular doctrine of life. At any rate we rest in the hope that, whatever view we take of the nature of life, or the origin of species, be we vitalists, or physicists, or evolutionists, it does not follow that intelligence and design should be banished from the cosmos, and we surely are not forced to do what thoughtful heathen—Galen, Cicero, Seneca, and Plutarch—were zealous against. The argument derived from the evil, imperfection, and pain, existing in the world (the great mystery!) used against design, may become a dangerous one, and be carried to greater lengths than is desirable. Though early impressions are, we are told, of a very generally erroneous kind, yet it appears a very strong one, and almost bordering on the innate, that all that we see beautiful and adapted in nature is attributable to design and not to the fortuitous.¹

Granting the truth, with the above *proviso*, of natural selection and of development from the simple to the differentiated—of the derivation of one from another—we may inquire whether species and genus have any real existence as such, or are they merely abstractions of the mind of the systematist? Is not the essence of species, in this case, the form rather than the object, and is its non-reality proved because that form may change to some other under certain circumstances? We presume that in forms avowedly liable in their nature to variation, yet, in the main, and in the majority of the individuals, constant, as far as

¹ The wonderful and admirable correspondence of organizations with their surroundings and of organs to their functions is admitted by most, though some affect to consider it as sometimes imperfect, or bungling, or at any rate as the product of no highly qualified or beneficent workman, but a mere matter of course. We may ask such to point out better modes of accomplishing these adaptations and functions, or to tell us what would be their feelings were any fault or derangement of nerve, vessel, duct, or membrane to occur in their bodies. The *derivation* of these correspondences is what has been principally disputed. That they have grown up in *liaison* with the environment we are willing to grant; that they are from the mere sifting of congruities and incongruities we doubt; and still more that they proceed from the working of mere molecular force.

we can see, to certain characters, it is philosophical to consider the common type as a reality, and not the less if it should ever happen that the variations of a second type trenched upon and assimilated with those of the first. Looking back through geological time organic types certainly seem to have constantly appeared and disappeared on the earth, and, upon the whole, the simpler forms seem to have been succeeded by the more perfect, yet the geological record does not clearly and fully show a gradual transformation, apparently pointing out that either itself is very imperfect, or that advances of a more sudden character must have occurred from time to time, either from meteorological or geographical changes, in the *instance of generation*, or from some other unknown cause. In our present vista, species, though subject to variations, appear in a sense unchangeable, so far that to the prevalent forms from which they diverged the varieties have a strong propensity to return. Such a tendency is even seen in the common pigeon in all its vagaries from the rock dove, and, besides, the pigeon type seems indelible in these extreme variations. Without any pledge to a Platonic system of idealism, therefore, it may be allowable to believe that species in the animal kingdom are in one sense realities, or whence come the above permanence and reversion? There here appears a real difference between species and variety, and a difference which does not seem well accounted for; besides we have the equally generally true and well-known fact, that though two varieties may breed together normally, yet two different species, however near, very rarely produce fertile offspring. Species, too, continue for an immense time in spite of causes of disturbance, constituting stereotyped phases of development. The same species of *Megalichthys*, *Palæoniscus*, or *Platysomus*, seem to be found through a series of coal strata a mile in thickness, and the writer has sometimes been struck with the circumstance that on the same shore, or brought to shore, it may be in the same fishing boat, may be seen numbers of siphonostomatous molluscs, *Buccinum*, *Fusus*, and *Voluta* for instance, animals of distinct but closely allied form—of the same habits, one would suppose, and of the same geological age, yet, though liable to varieties and of wide distribution, they have continued to retain their typical character for vast ages as far as we can ascertain. Again, though most thoughtful minds have imported the doctrine of gradual development or progressive metamorphosis of some kind into biology and physiology, that is, as the law of action of a special power which is neither allied to the ordinary physical agents, nor the result of the whole series of them,—yet the apparently simultaneous origin of all placental mammalia with the Eocene is remarkable, and seems to point out

some more rapid introduction of forms on the earth at this period than could be brought about by an uncontrolled evolution or natural selection. At this point the geological record must be imperfect indeed, for representatives of all the mammalian orders seem to appear at once—*Balæna*, *Ursus*, *Equua*, *Sus*, *Cervus*, *Bos*, *Elephas*, *Phoca*, *Canis*, *Felis*, &c. (Linn.); our present flora also being contemporaneous. The Cephalopods of the more ancient strata have conversely disappeared and left little trace behind of their wondrous forms, and so with the reptilian life of the Mesozoic. Sir W. Logan says, with respect to the Eozoon, “that the distance of time separating the Lower Laurentian, where the fossil occurs, from the Upper Cambrian may be as long as that separating the latter from the nummulitic limestone of the Tertiary; yet during this vast period Evolution apparently was reposing; *Trilobites*, *Oldhamia*, *Lingula*, *Orthis*, and worm-tracts constitute all the signs of life to be found. As far, therefore, as the, no doubt, imperfect record goes, Evolution would appear to have had its periods of inactivity as well as of activity.

Driven into rougher ground from our pleasant pastures of the belief in the sustention of living beings by an intelligence, or in their being subserved by an innate principle of life, in itself nearer to intelligence than any blind physical force,¹ we are of necessity led to examine, a little further, the doctrines of evolution, which profess to explain, as already observed, the first appearance of organic forms as the result of chemico-molecular force in highly unstable compounds, and the further modification of the same forms as caused by direct or indirect equilibration,—that is, the incidence of external and surrounding agents upon them, and their corresponding response. Evolution, its disciples say, can be inducted no higher; the formation of a primordial organism is but analogous to the formation of a crystal or a septarium; consequently, Evolution apparently implies a kind of heterogenesis.

It is the environment that causes one surface of an animal to show the dorsal characteristics, the other the abdominal or pedal. Progression of any kind entails that the anterior part becomes the head, the opposite the tail, and symmetry is of course largely dependent upon the capability of progression. The jointed form of the articulated animal is due to the process of division or budding, so common in the lower animals, different from the jointed disposition in the vertebrata, where it is merely adaptive, and brought about by simple mechanical means, just as we see, when we bend a stick of warm sealing-

¹ “A single power through the whole, and that a living one.”

wax, a tendency to crack. *En passant* it may be observed that if this doctrine respecting the articulata be true it deducts from the difficulties of a serial arrangement of the animal kingdom. But the limitation and fixedness in the number of joints, and the peculiar character of definite sets in the chain, seem to show that the doctrine is incorrect. The difference in the claws of the hermit crab is said to be an instance of form being determined by external incidence; but this perhaps hardly explains the difference of form in those of the lobster, though the animal's voluntary elective use of them may do so. The other organs have an equally simple and mechanical origin; so the digestive organs, commencing with the involution of the endoderm within the ectoderm; so the lungs, an advance upon gills, brought about by what we may see in confined gold fish, or in the fish of a pond, whence the water is gradually drawn off; such fish rise to the surface and take in by the mouth a quantity of air, thus imitating lungs. Of course such views as the above are incompatible with the doctrine of the archetype vertebræ.¹ The formation of vessels is explained upon mechanical principles, but that of the central organ, the heart, puzzles the evolutionists, and so do nerve and muscle, as already alluded to, their action due to the rapid changes which highly unstable matter, whether colloid or not, is capable of, though contrary to its usual rather slow changes, but now, perhaps, connected with some isomeric action.

The size of animals has generally been considered as specially adapted to the circumstances in which they live, not omitting from the consideration gravitation, strength of material, and such like; the evolutionists explain it more simply and mechanically with reference to the supply of spare nutriment, the force expended in other directions than in growth, and the size of the egg or foetus.²

Evolutionists argue that many species are notespecially adapted for their present habitats, or rather that other species might

¹ It is now said, we believe, that traces of segmentation are found in the basal cartilage of the skull from the first. But much more than this segmentation is implied in the formation of the vertebral column.

² The egg of the cuckoo ought to be three or four times as large as it is, considering the size of the bird. Though a bird will sit on a stone, yet it must be somewhat like its own egg; and so the cuckoo's egg resembles that of the much smaller bird in whose nest it is commonly deposited. The bird sometimes oviposits in the nest of the hedge-accentor or the whin-chat, both of which have the eggs blue, like those of the American cuckoo which builds its own nest. There appears design here, but at the same time something like the derivation of species. Mr. Spencer says that actiniæ do not dwindle if not fed; this we think is an error with some species. Small perch (as well as the small male salmon) have sexual development, and Mr. S. discusses, and his discussions are full of interest, which is the natural state of the species, the small or the large size.

thrive better in such localities; for instance, English weeds have been introduced into New Zealand and flourish so well that they threaten to exterminate native vegetation, a reason, one should suppose, for their not having been planted there by nature. European animals threaten to displace the Marsupialia of Australia, but the first are protected by man, and it has also been shown, we think by Owen, that the latter are especially adapted for such a country, that is as feral animals, where at times food and water are only procurable at very distant spots to which the unaided young could not attain.¹

In vegetable morphology many structural arrangements are ingeniously explained by evolution, but often on very mechanical principles. Some of them must have been evident to most observers, though they may not have looked upon them as *veræ causæ*, for instance, the formation and condensation of the woody axis in di- and monocotyledonous plants. As regards the latter—the stem of which is formed by the sheathing and closing leaves—the evolutionist would not sympathise, we suppose, with the old Italian philosopher who, as the story tells, when summoned before the Inquisition for heresy of a deep dye, took a straw which clung to his garment, and exclaimed that the consideration of the structure and function of that straw convinced him of the existence of a God. A straw is, indeed, like many other things in nature, an example of adaptation. No principle is said to be evinced in the morphology of the leaf-stalks of plants; they are commonly, however, more or less like the letter v in section, hollow or filled up, and such a form appears well adapted for strength, and to support the leaves in the horizontal position; no doubt the leaf-stalks have many modifications, such as the round one of a peltate leaf, the compressed ones of the aspen, or the winged and tubular supports of the *Calathea*.²

In all the above examples we may doubt whether the supposed agents are sufficient for what is attributed to them—whether evolution contains more of reality than the superseded vital force, or whether it be not the mode rather than the cause. Another factor seems wanting for the appearance of life upon the globe and for its maintenance;³ at the same time credit must be

¹ That like organisms are not generally found in like habitats, appears to tell as much against evolution as against design.

² Why have the aspen, the white poplar, and the abele their tremulousness, and the two surfaces of the leaves strongly contrasted? Is it to attract the little *Melasoma*, which abounds on them, as well as *Lepidoptera* and other insects, or to give life to the dark and damp flats on which they grow?

³ "You will get out of your atoms by 'evolution' exactly as much and no more as you have put into them by hypothesis. Science teaches the 'how' and theology the 'whence.'" "Modern Materialism," J. Martineau, 'Contemp. Rev.' Feb., 1876.

given to the equilibration, or the mutual correspondence of organisms and the environment, to the survival of the fittest and the extinction of the unfit, as well as to development or derivation in the economy of nature. But admitting so much, we discard the theories of the aimless and fortuitous construction of animals, and demur to accept the theory that life is but a form of physical force, or to renounce the teleological doctrines of design, seeming in the last case to give up the *à posteriori* arguments for what we have considered as one of the first principles of belief. Where there is room for incertitude, however, it behoves us to admit that truth is more sacred than all:—

“Quid si ruat cælum?”

It may be observed that Dr. S. Clarke, who has probably written on this point as well as any man, admits that it might be held that “infinite wisdom, foresight, and unerring design originally so ordered, disposed, and adapted all the springs and series of future necessary and unintelligent causes that, without the immediate interposition of Almighty Power upon every particular occasion, they should regularly, by virtue of that original disposition, have produced effects worthy to proceed from the direction and government of infinite wisdom.”¹ So writes Dr. S. Clarke, and though he was suspected of minor heresies, he never was on these points.²

Confining ourselves to what we are most familiar with—rather humble creatures—we may give a few examples of what appears to us as something more than undesigned equilibrium of the organism and its exterior; but in this, as in other subjects, mistakes may be made, and it would not be fair for any such misapprehensions to affect the issue of the case; moreover, in the interpretation of facts there may be a quota of truth in different views.³

¹ ‘Discourse on the Being and Attributes of God,’ 1749, p. 159.

² “Irregularity of method is a mark of weakness, uniformity of method a mark of strength. Continued interposition to alter a prearranged set of actions implies defective arrangement in these actions. The maintenance of these actions, and the working out by them of the highest results, implies completeness of arrangement.” ‘Principles of Biology,’ vol. i, p. 354. Mr. Martineau compares the uninterrupted reign of law in the universe with the automatic actions of intelligent beings, primarily originating in their will.

³ To affirm *purpose* in the adjustments of the world is not to set up a rival principle outside their producing force, but to plant, or rather to leave an integrating thought within it. And conversely, to trace these adjustments to their “physical causes” is not to withdraw them from their ideal origin, but only to detect the method of carrying the inner meaning to its realisation. Who will venture to say, what nevertheless is constantly imagined, that to find how a change comes about is to prove that it was never contemplated? If it were contemplated, it would have to be executed somehow. J. Martineau, ‘Contemp. Rev.,’ March, 1876.

There is, perhaps, as little in the Mollusca as in any class of animals of the mechanical and symmetrical, and yet enough, we think, to claim our notice; and, first, we will ask how is their protection provided for, and especially that of the branchiæ? for the action of oxygen on these branchiæ is of tantamount importance with nutrition, nay, more so, for its want is more immediately felt; it is only by respiration that the gross product of nutrition is, by a kind of reverse process, freed from elements which are not assimilable, or worse, noxious, and at the same time refined to a net product, fit to sustain life, and especially the nervous and muscular systems. Here we must bear in mind that it is only their habitual and normal protection that is provided for; many of them being required for the food of man, of birds, of fishes, and of other molluscs, are destined to subserve the general good. There is no more massive shell on our coast than *Venus Islandicus*, with its closure strongly secured, yet the sea-cat (*Anarrhichas*) is able to crush the valves with its powerful teeth, and discharges the pieces afterwards, very imperfectly comminuted and liable to be mistaken for the similar fragments found in the drift. So, probably, the strong but beautiful *Trigonia* is amenable to the powerful jaws of the Port Jackson shark; the representatives of both shell and fish also existing in the oolitic age. Ordinarily, safety and protection are well secured in molluscs by their shell coverings, which in bivalves open by means of an elastic substance, variously disposed at the hinge; yet are they quickly closed if need be by internal muscles; the soft-bodied tenant being also often guarded by argus-like eyes disposed along the slit and shining like emeralds,—probably light, but not object, organs, and giving warning for closure to the strong janitor muscle when any shadow falls upon them. Such is the disposition in the pecten or scallop, where, moreover, as commonly the most concave valve lies undermost, the consequence is the animal settles down on it in the water. The smaller species of scallop, with more equally hollowed valves, have very active movements caused by the contraction of the adductor muscle above mentioned, and the sudden closure of the valves. A few placed in a pail of fresh water soon leap out. The security of this and numerous other shells is increased by the ribs and ridges of the valves locking into each other, but primarily this security is ensured by the hinge itself, much varied and very strong, and its processes or teeth admirably fitting into each other, often like a butt hinge, as in the spondyle shell or clam. The ligamento-cartilage or elastic substance opening the valves is remarkably varied in different species and for different ends.

In pecten it is internal, and simply acts like a piece of compressed caoutchouc, opening the valves widely by regaining its natural state, which here may be done safely from the short dorsal junction of the mantle. In the thin and brittle *Anatina* it rests between two spoonlike processes, buttressed with ridges, the only way of obtaining a firm foundation on the fragile shell; and so disposed, its action is more limited than in the former case, neither allowing the shelly valves to constrict the large respiratory siphons by closing too much, nor to tear the united sacciform mantle by a too great divergence. In other species the so-called cartilage may be half internal, half external, and act both by tensile and resilient elasticity.

In the common *Anodon*, or the *Iridina*, it is such a hinge as would be formed by doubling up a piece of strong leather. The teeth adjunctory to this hinge vary infinitely; in the oyster they are obsolete, for the adductor muscle is strong enough to guard the animal from everything but the oyster-knife. They are large and beautifully fitted into each other in *Trigonia*, though often deficient in other cases where there is the same interlocking of the ridges of the valves, or if the shell is small, flat, and polished, and so exposed to little violence. *Arca* has a long straight hinge, with rather weak ligamento-cartilage, but the teeth are very numerous and saw-like; and the umbones of the valves far apart, to allow of their wide opening and of the protrusion of the massive foot. This arrangement necessitates the division of the heart into two ventricles. Beautiful as the teeth are in *Trigonia* or *Nucula*, they are formed by a simple interposed portion of the membranous mantle, more glandular where the cartilage has to be secreted. That the scallop is an inhabitant of deeper water than the oyster or mussel is shown by the open spaces or imperfect closures near the ears of the shell, through which in some species a byssus makes its exit, mooring to the rocks the otherwise detached animal. An incomplete closure would be fatal to an animal liable to be exposed to the air from the concentration of the sea-water. When it attains the saltness of thirty grains of salt to the ounce it becomes a poison, whilst the action of fresh water is the same; the naked *Doris* crawling on the rocks has the power at ebb of tide of retracting its delicate ciliated branchiæ from the sun or rain, or with other naked-gilled molluscs will lurk under the wet algæ. The limpet, in hot days, sticks firmly to the rocks so as to prevent the drying of the confined moisture. Those bivalves which, like the common mussel, are exposed on the bare shore to the action of the sun and rain, have the valves fitted to each other most exactly, preventing all evaporation; other species burrow at the retreat

of the tide into the sands, and so obtain protection for their ciliated structures.¹

Shells, though often so beautiful, are but dead exuvia, and consequently the growth of the contained animal can only be provided for by fresh laminæ successively enlarging, and secreted between the fleshy matter and the old layers. From the growth of the animal it follows that all shells have a minute nucleus to start from, and are wonderfully varied in form as growth proceeds. Univalves have essentially conical shells, covering the branchiæ and viscera of the animal, and taking on the spiral or volute form as they increase, and this on the same plane or not, producing the very pleasing and infinitely varied form of these shells. Coloured as they often are in a beautiful manner, we cannot but suppose that there is here variety and beauty introduced for their own sake; in fact, there appears very little beauty in the world but what is immediately or mediately connected with organic objects. It is pretty evident, at any rate, that sexual selection can here have no hand in the matter.

It is the circumstance of the many modifications which the shells of molluscs are capable of that excites interest—*modification* rather than *development*—in one direction. We see the ordinary bivalves covered by their shelly plates, as a book is by its binding, now secured at the back as a book is, now strengthened in front by projections and corresponding sinuities, and very varied in their general form;—fan-shaped and pointed below in the Pinna, by which means the animal lodges itself in the sea-bottom, further securing itself there by its beautiful byssus, whilst the common mussel, a somewhat allied mollusc, becomes firmly packed into the tessellated bosses which we see on the rocks;—acting as splints for the long bodies of the razor-shells which burrow deep in the sands by means of their very effective foot;—modified into an augur in the ship-worm and disintegrated so as to form besides a long cylindrical dwelling, the palettes acting as a door. In some, one valve may preponderate and form a sessile and firmly fixed cup, whilst the other becomes a well-adapted lid, as in many fossil forms called Rudista, and certainly some of these seem almost rude and rough enough to be the product of some obscure physical force. In the Brachiopoda or lamp-shells the valves are curiously transposed, or, at any rate, arranged in the opposite plane to those of other bivalves, and the lower valve is often attached by its elongated beak to the rocks, and in this case also the other valve forms a cover, whilst within is a very curious

¹ 'Mag. of Nat. Hist.,' 1833,

mechanism, too varied and complicated for us to attempt to describe. Then, again, the coalesced shells of the bivalve form the conical dwelling of the limpet, or that of *Dentalium*, shaped like a perforated elephant's tusk, and through such forms as the ear-shell or *Haliotis*, *Emarginula*, *Calyptraea*, *Dolabella*, &c., the spiral form is attained; and, finally, one side becomes atrophied, and consequently one valve absent, except as the curious operculum, still, however, useful to close the mouth of the spiral cavity.¹ Some shells are beautifully light for natation or floating, others heavy and spiny for rocky shores. The remarkable shells of the extinct *Cephalopoda*, probably in many instances, rendered buoyant the animals which formed them by their hydrostatic cellular structure.

Growth in shells can only take place in the way mentioned, by the deposit of new layers, and the shell itself being extra-systemic can afterwards be but little modified, though it is so to some extent by similar modes to those by which rocks, &c., are worn away by these animals; a tubular shell may lengthen but cannot expand, though the shelly coverings of some cirripedes are capable of disintegration in certain lines and so may enlarge their cavities laterally. All this, no doubt, can be made assimilable to evolution, but we see no ground to suppose that the influences of the environment are the sole factors.

If security be accomplished in such shelly domiciles, how is locomotion performed in such molluscs as have it? for some are absolutely fixed to the rocks and to other bodies in various ways, of which we need only mention that by a byssus, the strong horny threads seen when we open a common mussel. These threads are formed from the secretion of a particular gland situated behind the front muscular attachment of the foot, and into the groove of this tongue-like organ the gland pours its secretion, where it is moulded, and afterwards attached to the tendinous point at its base. This foot is very extensible as well as capable of casting or moulding the secretion in its groove, and hence of attaching the threads to the rocks, or to other mussels by little disks, matting all together in a firm mass. Very frequently this organ is even further developed, and in the bivalves it is attached by strong retractor muscles to the interior of the valves, comporting itself like a tongue, or foot, or finger, or proboscis. The movements of molluscs are, creeping in various ways as well seen in a common snail, swimming in the higher genera, but for this special organs are developed, and burrowing or boring. Though the last is perhaps *par excellence* the faculty

¹ This, however, is a disputed point of homology.

of the foot, yet it may also act as a spring, a crook, a boat, a clasp, an anchor, or even as a spoon; whilst the divided or differentiated foot or so-called arms of the cuttle-fish, with their curious acetabula or suckers, act as deadly grappling irons, though with a somewhat different mechanism in different species.

When this foot is not developed as in the *Ascidia*, the *Brachiopoda*, or in the oyster, the animal is indebted solely to its vibratile cilia, so common in the lower animals, and here covering especially the branchia and interior of the mantle, for the inhibition into that mantle, or pallial-chamber, of rapid streams of water, which not only convey from a distance the nutritive organic particles floating in the vicinity, but also aërate the branchia. Very beautiful is the action of these cilia when seen under the microscope, as beautiful and wonderful as the circulation; and very varied is the hydraulic mechanism of the mantle in the bivalves to direct the flow of the stream when it is drawn in or forced out; in fact, so varied that it is not difficult to classify them from these arrangements alone.

In the foot of bivalves is a peculiar body called the crystalline stilette, respecting the homology of which perhaps something might be said, but of which a simple use is to act as an internal support, giving resilience to the foot in its various movements, and helping to restore it to form after retraction; it appears to confirm this view that it is very long in the *Anomia*, and here is lodged in a promontory of the mantle, which in this animal is free and with little to keep it in position.

We have alluded to the modified foot or prehensile arms of the *Cephalopoda*. Here the organs of locomotion and of respiration are connected together, and undergo several modifications in different species. On the wet rocks the *Octopus* uses its long arms for progression—climbing or walking, carrying its flask-shaped body above; but swimming also is accomplished by the expansion and closure of the circle of arms in unison, this being made more efficacious by their being more or less webbed at the centre; here progression is by starts, the arms trailing behind. In the *Sepia* the water is taken in by the wide fissure seen between the head and the body, produced by the detachment of the fleshy mantle or envelope of the body; the mantle expands like a common india-rubber bottle in order to draw in the sea water. When the water has served its purpose the mantle contracts, and two large folds or valves at the base of the neck now rise and prevent the water from leaving by the same fissure, but allowing it to escape through a fleshy siphon or funnel, having itself a valve in the interior to

prevent the entrance of the water that way. Through the funnel the ink and *rejecta* also pass out, and to keep the funnel, valves, and mantle in position during these acts there is an extremely curious mechanical contrivance; there is on each side a ball-and-socket joint formed in the base of the funnel and on the mantle, but capable of temporary dislocation.¹ These respiratory movements drive the animal backwards, but it is not solely dependent on the above provision for locomotion, for it has, in addition, two lengthened fins at the sides of the body, by means of which it can move itself in any direction in a very beautiful manner, often the head first and the back upwards; this last position being secured by the lodgment in it of that light spongy hydrostatic body called the cuttle-fish bone, no bone at all (though the animal has such, having a resemblance to the internal skeleton of the skate), but the homologue of the hydrostatic cell system in the Nautilus, and of many extinct and curious allied shells. The Loligo is also well organised for locomotion, shaped like an arrow for progression, and also capable of springing out of the water; whence the sailor's name for it, the *flying squid*. Their ink is also another means by which the Cephalopoda evade their pursuers, as they can, by discharging it, discolour a large quantity of water. The analogy of the longer or tentacular arms of the Onychoteuthis with certain surgical forceps, constructed with an *extempore* joint, has been pointed out—the grasping parts at the ends are armed with hooks, but lower down they are capable of being locked together by means of unarmed suckers or cups; many Cephalopoda have two very long supernumerary arms by means of which they can anchor or tether themselves, at the same time allowing a pretty wide exploring ground.

Many are the modifications, as already observed, which the molluscous type is capable of; not the least remarkable being that of the borers into the hard rock, taking the external form, to an ordinary eye, of a worm or an annelid, but their own type in all respects remaining: the Aspergillum is very curiously changed, yet it has still the characters of a bivalve mollusc. How the boring of hard rock or wood is accomplished is matter of dispute; nature has more ways than one of performing its task. In some cases there is, no doubt, mechanical work done by means of the valves, in other cases this cannot be; in some there is attrition by the fleshy foot, aided perhaps by gritty particles; in the ship-worm (*Xylophaga*) and wood-pholas there is a pore in the centre of the foot to which the crystalline style is directed, and the pore may give exit to a

¹ The branchiæ of the Cephalopoda are not ciliated, 'Trans. Linn.,' 1834.

solvent fluid from the stomach, so aiding the mechanical action of the valves—at least of those of the ship-worm. But in all these operations the strong currents, produced by ciliary action and directed downwards, and the never-ceasing effect of the applied ciliated foot, are probably prime factors—

“Dura tamen molle saxa cavantur aquâ.”

Unlike the earth-dwelling molluscs, the little light Pteropoda swim through the clear ocean, in much the same way as butterflies sail in mid-air, but here the above-named mantle is modified into two fins or wings,¹ whilst the foot is undeveloped, the wings exhibiting as beautiful tints as those of the insect. Sometimes a shell exists in these, and, of course, it should be light, which it is, and often beautiful; sometimes boat-shaped as in Argonaut, evidently organised for surface or mid-ocean, though the animal can also creep on the rock. Great is the beauty of structure and adaptation to its shell in this creature; though for long it was debated whether the animal found in the shell is the constructor or not. We can well believe that this little creature is really alluded to in the grand song of praise :

“There go the Argonauts, and there is that Leviathan whom Thou hast made to play therein.”

Omitting other interesting points, the minute and extremely varied lingual apparatus of the higher molluscs; the branchial appendages of the nudibranchs; the heart and vessels, the latter more complete than has been thought; the digestive organs, with the extraordinary armature of the stomach, and the beak of the cuttlefish, we will pass on to instance a very few points from the Articulata.

Réaumur's volumes constitute a mine of information respecting the wonderful habits and structure of insects. We shall only supplement them with one or two observations on points of structure, where we are disposed to believe that prospective adaptation is manifest, and that in Crustaceans, not perhaps the most curiously organised of their class. *En passant* we may observe that the metamorphosis of insects appears to us to instance that change of development which all organisms in a greater or less degree avowedly undergo, and to prove its existence; it is too an instance of a kind of origin of species and that before our eyes, but this not brought about by the action of environment, though in relation with it, aerial, aquatic, &c., but regulated from a *force from within*. We must also have, in this case,

¹ But formed by the sides of the foot according to others, or its epipodia.

faculty or force inherited, rather than the peculiar molecules of the panspermists; unless we can suppose the egg to contain at once the germs of the grub, the chrysalis, and the imago; and these molecules to have a wonderful elective power of segregation, and as wonderful an aptitude to abide their respective time.

Leaving, then, the world of insects, their countless numbers of species, their exquisite but microscopic structure, the adaptations of their structures to use, their habits as regards their own preservation and that of their offspring, the relation of life of one species to that of some other or to some particular plant, their consentaneous faculties, and the formation of their habitations, we take a glance at another order, the Cirripedes, which Articulata may be arranged with or next to the Crustacea, and being a tribe of animals which the author of the 'Origin of Species' has almost made his own by his researches; we may give as our instance the eye of the *Lepas*. In the free swimming larvæ there are, at one stage, two eyes, but in the perfect animal, fixed by its pedicle, and with its body inclosed in an opaque mantle, and between shelly valves except at the chink in front, there is but one, situated, however, immediately behind the chink, and evidently sensitive to the light, like the mole's eye simply a light—and not an object—organ; it may be seen that the animal closes its valves upon every shadow passing over. Now, this eye lies deep in the body upon the stomach, and some have supposed that there is no possibility of the rays of light affecting it. This, however, is not the case, for a scrutiny will lead us to observe the little fact that in the loose external dark-coloured tissue, enveloping the animal's body, there is just at the point before the eye-speck a clear lozenge-shaped space, through which it is seen, and through which light can easily penetrate.

The moulting or ecdysis of Crustacea is a necessary provision for the increase or growth of the animal. Without entering into the full history of this wonderful operation, that is, how it is effected in all respects, it may be mentioned that though Réaumur says that there is a splitting of the shelly covering of the large claws in the lobster, crab, and river cray-fish, to enable the voluminous soft parts to be withdrawn through the narrow base of the limb, yet generally this has been denied. But it is a fact that a splitting of the shell, at the narrow part, through the second and a portion of the first joint from the body, does take place, though in the exuvial shell this is not readily seen, as its elasticity restores it to form after it is cast off; but a minute examination will prove the fact; and, what is curious is, that in all crustaceans with these large fore-feet there is a *pro-*

vision for the moulting, a line of attenuated shell of a particular pattern, along which the shell always does yield, never giving way in any other line than in this. In the crab the body-shell is exuviated at the line marked on the sides.

The casting off of the limbs of these animals, when injured or trapped, or as it is said when frightened, is also noteworthy; and appears to us to be equally provided for, so as to be accomplished with safety. The separation does not take place at the joints, where the members of the limb meet, for in that case the tendons, being attached just above the junctions, must be forcibly torn from the interior of the muscles below, inflicting much injury, and probably death; but it takes place at a marked line at the narrow part of the limb (in the crab or lobster), at which line no tendons can be drawn out, for they are inserted just below the line of separation; neither is there any laceration of muscle, for none exists at this point, but only a formative plasm, (as was pointed out by Dr. Goodsir,) ready to commence of itself the formation of a new limb, and through which pass the nerve and vessel of supply. The muscular power exerted to cast off a limb must be great. In the separation a *point d'appui* is formed, by the opposition with the first joint of the process which projects backwards from that part of the second joint, which is beyond its contracted portion and the line of separation: then the intervening narrow part is acted upon with a leverage by the muscles inserted in its interior, but arising below in the body of the animal, this part which is retained being hinged as it were upon two opposite points of the joint below. The line of fracture is also marked within as well as without; and when examined, this fracture presents a different structure from what a fracture elsewhere does, for it has a transversely fibrous appearance, and a thin film of soft tissue extends from surface to surface.

It may be noticed that there is a difference in the fore limbs or mandibles of the lobster: one, which may be the right or the left, has a cutting edge with pointed teeth to the pincers, and is used for various purposes in feeding, &c., whilst the other is shaped like a pair of grappels with blunt tubercles, and is fitter for the animal to anchor itself by, or to crush shells, or other bulky or specially hard food.

In conclusion we may give the following brief summary, omitting a few minor points, of the preceding remarks. First, we have recurred to the doctrine of the vitalists—of such men as John Hunter and Sir Charles Bell, not to mention other more precise thinkers—a doctrine denying that vital force can be reduced to any form of the chemical or magnetic, much less of those other forces, such as heat or electricity, which circulate

in so unconfined a manner through the cosmos; but affirming that it is, as far as we can see or define, a more limited *energia*, indwelling, yet ever expanding, and oftener in opposition—rather than subjection to ordinary physical force. Secondly, we have endeavoured to show that the vital force, though autonomic in the above sense, is subservient—and, perhaps it may be said, more intimately related to a higher intelligence in nature; and to this, though pre-ordained law is ever adhered to, we would attribute the wonders of organic structure, as well as that beauty and adaptation in natural objects which all men, whatever their mental tone, do not fail to admire.

II.—The Connective Tissue Question, Illustrated by a Consideration of the Present State of Opinion on some Disputed Points.
By GEORGE THIN, M.D.

READERS of the histological essays, which occupy so prominent a part in German scientific literature, are familiar with the term *Bindegewebsfrage*—the connective tissue question—and the tacit avowal which it implies. But in the same breath in which we throw doubt on our knowledge of the normal structure, we imply that there is no basis on which pathology can find a solid resting-place; for it is beyond dispute that those who differ regarding the histology of connective tissue have no common ground on which such processes as inflammation and cancer can be profitably discussed. It would be as reasonable to expect students to agree in their interpretation of the hieroglyphics of a lost language to which they applied different keys, as to expect pathologists who, for example, are not in harmony as to what constitutes a cell in the natural condition, to agree regarding the changes which take place in cells which are diseased.

The subject is thus interesting in the widest sense, and however puzzling the dogmatic assertion of irreconcilable opinions may be to those who are in the position of lookers-on, yet no one who wishes to follow the changing phases of contemporary medical science can afford altogether to overlook it.

The limits of a short article do not allow me to attempt anything like a history of the present state of opinion on even any one section of this wide subject; but I shall try to give such an account of some of the different views that are held regarding a few of the most commonly discussed parts of it as will make it clear that there is very little that can be considered as generally accepted in some of the fundamental departments of histology. I will then try to show by some examples that this uncertainty depends on want of precision in our ideas regarding some of the substances which may be considered as simple elements of the tissues, and above all by the difference of opinion that prevails regarding the interpretation of the effects which are produced by reagents that are now in constant use in these studies. A more special reference to some points that have been lately raised will serve the double object of calling attention to several questions that excite an unusual amount of interest at the present time, and will also show that histological doctrine is still dependent on the results of methods regarding whose *modus operandi* there is anything but unanimity.

My object at present is thus neither to judge nor to criticise, but to show the nature of some of the causes which produce the existing differences of opinion amongst histologists. This end may perhaps be conveniently attained by considering in succession several of the connective substances which have been much studied during the last few years, and more especially the nature of their cellular elements and the relation which these have to what is termed the fundamental or ground substance.

In order to limit the range of this retrospect, I shall only consider some of the memoirs which have appeared since the publication of Rollett's comprehensive chapter in Stricker's Handbook, and I can, of course, give no more than a bare outline of the views of the authors to whom I shall refer. I shall begin with Ranvier's memoir on tendon in the '*Archives de Physiologie*' for 1869.

Previously to this time Henle had taught that the cells of tendon persist as flat plates, which are arranged in rows in its long axis, the plate-like bodies being without nuclei. Virchow, on the other hand, taught that the anastomosing stellate figures which are seen in cross sections of tendon are the cells of the structure,—the connective tissue corpuscles of that author. Henle maintained against Virchow that these stellate figures are simply spaces left between the cylinders of tendinous substance that compose the body of the tendon.

The merit of Ranvier's memoir consisted in his showing that Henle's rows of plates are nucleated cells, and in the description of ingenious and simple methods by which that fact can be easily demonstrated. He further insisted on the truth of Henle's destructive criticism of the connective tissue corpuscle of Virchow.

In this first paper Ranvier expressed his belief that the cells curved round on themselves until their opposite lateral borders touched each other, and that a number of cells being arranged in this fashion one above the other form a system of tubes in the interior of the tendon, an idea which, however, he afterwards abandoned.

The next contribution on the subject in point of importance is Boll's memoir in Schultze's '*Archiv*,' vol. vii. This author agreed with Ranvier that the cells of tendon are four-sided nucleated bodies, but showed that instead of being rolled on themselves to form tubes, they are applied to the surface of the bundles.

In foetal tendon, in which the cells are easily demonstrated, he believed that they embrace the bundles to about half their circumference, the bundles being thus contained in half sheaths, and further described longitudinal striæ in the cells, which he supposed to be connected with the formation of elastic fibres.

He found that in adult tendon the cells had become incorporated into an elastic sheath, which is firmly adherent to the bundles, the latter being further penetrated by elastic fibres, which take their point of departure from the cell-plates of the sheath.

In the 'Quarterly Journal of Microscopical Science,' vol. xii, Dr. Mitchell Bruce published a paper on the "Structure of Tendon," in which he confirmed Boll's statement that the cells are applied to the surface of the bundles, and that each bundle is surrounded to the extent of half its circumference by one row of cells. But he added to the description of Ranvier and Boll a statement that in the adult tendon the cells are connected to each other by processes. "In the growth of the fibril bundles," he remarks, "in length and thickness the cell-plates become removed from each other, yet continue their mutual connection by communicating processes corresponding to the prevailing growth of the fibril bundle; the cell-plates will also be longer than broad, and will similarly come to possess longer processes in the long axis of the tendon than in the other." This conception of anastomosing processes is not to be confounded with that of the fibres which Boll believed to be formed in the cells, and is in direct opposition to the opinion of those who, like Ranvier, believe that the tendon cells have no processes, and do not anastomose with each other.

Spina, in the Vienna 'Jahrbücher' for 1873, described the tendon cells as being in the embryo, and in very young animals arranged in rows. In the course of development a fibre forms round each cell. The fibres thus formed join laterally, the whole arrangement being compared by the author to a ladder. Further, processes are sent out in all directions, but chiefly transversely to the long axis of the tendon, from the "ladder" and from the surface of the cell, and several strongly developed fibres or threads (*Faden*) may develop longitudinally on the surface of a row of cells. As the animal gets older the cells flatten, and eventually are found only as bands. From these cells and bands the elastic fibres of the tendon are formed.

Spina thus confirmed the existence of the fibre described by Boll, and added to it a number of others. (It may be as well to mention here that the majority of authors who wrote after Boll, Ranvier amongst others in a second memoir, denied the existence of the fibre, believing that the appearance described as such is produced by a longitudinal fold in the cell.)

The description of the fibres or processes connected with the cells as given by Spina is inapplicable to the anastomosing cell processes described by Bruce, and it is clear that these authors mean different things.

While Bruce only believes in connecting processes drawn out longitudinally on the surface of the bundles, Spina believes that a large number of finer processes penetrate the bundles.

In a subsequent paper in the '*Jahrbücher*' for 1875, Spina maintains the accuracy of his former description, and denies the correctness of a division of the tendon substance into primary secondary and tertiary bundles. Tendon, he remarks, is to be considered as being constituted by connected systems of cells in the form of a network with narrower or wider meshes, the meshes being filled by the fibrillary ground substance.

There is an indefiniteness about the nomenclature which is used in reference to the tendon substance, and before passing to the next author, it is necessary to make an explanatory remark on this point.

When a transverse section of a tendon is examined under the microscope, the surface looked at is seen to be intersected by a number of anastomosing lines which form an irregular network, the central knots or points of the network being generally somewhat lozenge-shaped and a little wider than the other parts. The cylinder of tendon substance enclosed by one such mesh corresponds to what many authors mean when they use the term *bundle*, although the term bundle is frequently used without an accurate limitation of its meaning.

Löwe writing in the Vienna '*Jahrbücher*' for 1874, on the '*Histology of Connective Tissue*' insisted strongly that this bundle, or as he termed it *Fibrillen cylinder*, is surrounded by a continuous sheath, and that this sheath is composed of the cells described by Ranvier, and an amorphous elastic ground substance which fills up the interstices of the sheath. Further, from thirty to fifty *Fibrillen cylinders* are enclosed in an endothelial sheath, the outer surface of which is composed of endothelial plates without nuclei, and the innermost of a sub-endothelial layer of nucleated cells separated from each other by an amorphous elastic ground substance. It being already generally conceded that the outer surface of a tendon is covered by an endothelial layer, Löwe maintains that the demonstration of similar layers in the yielding tissue which encloses the subdivisions of the tendon, and of the investment of the *Fibrillen cylinders* by Ranvier's cells, brings the structure of tendon into unison with the scheme of connective tissue developed by Key and Retzius in their work on the anatomy of the nervous system to which I shall afterwards refer, and which I may now shortly state implies that the bundles of connective tissue are invested by a layer of endothelial cells.

In the '*Edinburgh Medical Journal*' for 1874, I published a paper "On the Minute Anatomy of Muscle and Tendon," in

which I stated that I believed the bundles of tendon (*Fibrillen cylinders* of Löwe) to be composed of a number of smaller cylinders not previously described, and that these smaller cylinders, which I designated primary bundles, are covered by small narrow elongated cells. I also stated my belief that the ordinary bundle (or *Fibrillen cylinder*) is completely invested by the Ranvier cells. I further found a double layer of nucleated cells investing a division of the tendon which contained a great number of bundles. The cells of this sheath were irregularly rounded and arranged like an epithelium. It is probable that they are the same cells which have been described by Löwe as forming the layers of his endothelial sheath which enclose the group of thirty to fifty *Fibrillen cylinders*.

I had not then, nor have I since, seen anything like a branch or process connected with the Ranvier cells. I have always seen them in successful preparations as four-sided cells with even borders. But in the same memoir I expressed my belief that in addition to the cells I have mentioned there are cells with processes in tendon which are of a totally different type. I admit that the subject is one on which demonstration is not easy, and that there are many sources of error; but I have since described a method (in the 'Edinburgh Medical Journal,' November, 1875), by which the nucleus and part of the cell and its processes may, I believe, be detected without great difficulty lying transversely to the long axis of the tendon in the narrow space left between two of the Ranvier cells.

In connection with my assertion that the bundle of authors or *Fibrillen cylinder* is composed of definite subordinate cylindrical structures I may mention that Herzog in the 'Zeitschrift für Anatomie und Entwicklungs-geschichte,' 1876, has shown that in transverse sections of frozen tendon the bundle can be seen to be divided into a mosaic of well-defined fields, which correspond to cross sections of what I termed primary bundles.

Ranvier's last expression of opinion regarding the structure of tendon is to be found in his 'Traité technique d'histologie,' which is now being published. The woodcuts which accompany the text, leave no doubt as to his meaning, especially as regards the cells. These are described and figured as four-sided bodies applied in rows to the surface of the bundle. The bundle is only partly covered by the cell rows. The isolated cells which are shown agree in form with those figured on the bundles. Although four-sided, they are not as a rule quite flat, being furnished with depressions and corresponding ridges produced by the mutual pressure of the bundles. There is, further he tells us, a sheath round the bundle and from this

sheath fibres penetrate into its substance, but these fibres are not elastic fibres.

Lastly, Waldeyer in a memoir "On the Cells of Connective Tissue," in Schultze's 'Archiv' for 1875, gives the following description of the tendon cell:—"Let one, he remarks, open a book in such a way that the leaves are held in four, five or six groups which meet each other at different angles. That is the typical form on a large scale of a tendon cell." But the borders of these component plates of the cell are not evenly cut, but run into numerous fine threads, often of considerable length, which can join and effect an anastomosis with those of the neighbouring cell.

Such are some of the opinions at present held regarding tendon, especially regarding the cellular elements, for there are points raised by some of the authors whom I have cited which I have not thought it advisable to mention. There are other essays on the subject that have appeared since Ranvier's first memoir called the attention of histologists to the question, which considerations of space prevent my alluding to. It will be seen that few of the authors I have quoted quite agree with each other. *Tot medici, tot sententiæ*. I may also add that not one of them has given any sign that the investigations of the others have in any way affected his previously expressed opinions. Ranvier no longer holds indeed that the cells form tubes, agreeing with Boll that they lie on the surface of the bundles; but when we come to ask whether the cells have processes, whether they embrace the bundles completely or in part, whether the bundles have a sheath, whether there are fibres passing inwards to the substance of the bundles, what relation the elastic fibres have to the cells and the bundles, and whether the idea of a definite bundle at all is justifiable, we find nothing but the most discordant answers. If histologists were called to give evidence on these points before an intelligent jury a verdict would be impossible.

Let us glance at the state of the similar question in regard to some of the other forms of the tissue. Take the loose subcutaneous tissue for example. In an ordinary microscopic preparation of this structure stellate figures anastomosing with each other by fine lines are seen, and if a staining process has been used nuclei are seen in the stellate figure. These figures are the connective tissue corpuscles of Virchow, and as such are familiar to those who are acquainted with the writings of the great German pathologist. In the same memoir in which Ranvier developed his views on tendon he maintained that these stellate figures are not cells, but spaces between the bundles, just as he maintained that the similar fact is true for tendon.

The cells he observes in the 'Traité technique' to which I have referred have exactly the form of endothelial cells, that is to say they are thin, polygonal and regular; others have one or several prolongations. When seen in profile they are fusiform. Hence the origin of the conception of the fusiform or spindle cell. In the normal condition the cells are applied to the surface of the bundles, differing, however, from an endothelial investment in the following important manner—the cells do not touch other by their borders and large portions of the surface of the bundles are not covered by cells at all.

Not very long after the appearance of Ranvier's memoir, Flemming in an essay on the formation of the fat cell in the seventh volume of Schultze's 'Archiv,' entered at length into the question raised by Ranvier, and claims then, and in a subsequent paper, to have confirmed the views of the French observer. But it is by no means evident either from his description or his figures that he saw exactly the same appearance as that which is described by Ranvier. "The fixed cell of adult fibrillary tissue," he remarks, "has the form of an elongated, irregularly fringed (*gezackten*), and variously bent plate which contains either in the centre or excentrically a flat, elliptical nucleus surrounded by granular protoplasma, in larger quantity in cold blooded and smaller in warm blooded animals." Again, "the anastomoses of the flat cells with each other in the adult tissue I could not often make out with certainty any more than Ranvier did. They are always effected by extremely delicate colourless processes of the cell-plates, which tear very easily." In a second memoir 'On the Anatomy and Physiology of Connective Tissue,' in the twelfth volume of the same 'Archiv,' Flemming returns to the subject of the nature of the cells, and to a certain extent modifies his views. The result of what has been evidently a long and laborious investigation, is that the author did not succeed in distinguishing the borders or contour of the cells, and he has such reliance on the methods he employed that he states it to be, therefore, in the highest degree probable (*liegt auch die Annahme am nächsten*) that they have no well defined contour. He emphatically insists on the absence of anything like an endothelium or layer of connected cells, and quotes with approval Posner's statement that the boundaries of the spaces in connective tissue (*Gewebslacunen*) are formed by the tissues themselves.

A communication by Professor Axel Key and Dr. Gustaf Retzius 'On the Anatomy of the Nervous System' in the ninth volume of Schultze's 'Archiv,' contains an account of the researches of the authors on the structure of the subarachnoid trabeculæ. Each trabecula, they state, consists of a larger or smaller bundle of fibrillary tissue.

The bundles are ensheathed in a complete layer of endothelial cells. The elastic fibres lie between the bundle and the sheath. They may be removed with the cell-sheath, or the cells may be removed, and the elastic fibres left behind with an extremely thin almost homogeneous pellicle on the surface of the bundle. The authors make the principle of the formation of the trabecula of universal application. "A membrane (*Häutchen*) formed by two layers of extremely thin cells, between which run networks of bundles of fibrillary tissue—this is the prototypus in connective tissue." They defer the extended publication of the investigations on which they rely to prove this for the subcutaneous tissue.

The cells figured and described by Ranvier and Flemming, are characterised by the Swedish observers as "almost unrecognisable shreds of cell-membranes."

It is the professed object of Löwe in the memoir to which I have already referred, to prove that the theory of Key and Retzius holds good for tendon. He finds, as I have already observed, that the larger bundles of tendon (containing thirty to fifty of his Fibrillen-cylinder), are enclosed in a membrane which he compares to a serous membrane, and which consists of a superficial layer of non-nucleated endothelium and an under (subendothelial) layer of nucleated cells separated from each other by an amorphous elastic ground substance.

He claims to have shown the existence of a similar membrane in the subcutaneous tissue of the mouse.

In the proceedings of the Royal Society for 1874, there is a memoir by myself 'On the Anatomy of the Connective Tissues, Nerve, and Muscle,' in which I described and figured in the subcutaneous tissue of the mouse cells of two kinds; the one consisting of somewhat rounded or oval nucleated cells arranged in a layer, all the cells being similar in size and form, sharp contoured, and showing no trace of processes; the other of narrow elongated cells of which on account of the mode of preparation, all I knew was that they belonged to the subcutaneous tissue. I also figured and described appearances which indicate an epithelial¹ formation in the thin connective tissue which is found in the muscle of the frog, and in a subsequent communication to the Royal Society, I confirmed the previous observation by describing another method by which the same formation can be detected, and by which the contour of the individual cells can be clearly seen. In this

¹ For "epithelial" some writers would here use the word "endothelial." The best and ultimate criterion in language, use and wont, still leaves the application of the term "endothelium" to single layers of cells (Ranvier) a matter of choice. That authorities of equal weight can be cited in support of either does not diminish the inconvenience of the use of two words for the same object.

second memoir, I described a double layer of these cells observed in the perimysium of the breast muscle of the frog.

The existence of these cellular layers is a question which will probably much occupy the attention of anatomists for some time to come; and in my opinion the tone of forced sarcasm and antagonism with which it has been met in some quarters, is unjustified by the scant knowledge of histology which we can yet claim to possess. The part of Löwe's memoir which refers to it has been criticised by Flemming in the latest of the two papers to which I have referred, in a footnote which covers several pages. The learned professor in this note goes out of his way to make some remarks about my writings to which I may take this opportunity of alluding. In reference to Löwe's opinion that the perimysium is partly constituted by endothelial layers, he remarks that no one has ever described or figured a preparation showing these cells except myself, and to show how worthless my evidence is, he instances the following errors on my part. Referring, presumably, to fig. 13, plate x, in the *Proceedings of the Royal Society*, vol. xxii, he reproaches me with being so little aware of the modern views of the action of chloride of gold that I figured appearances produced by it in the cutis as branched cells. Professor Flemming is correct in pointing out that this was a mistake on my part, but that in no way affects the general accuracy of the view I hold that branched cells exist in connective tissue. That now rests on other evidence, in which no such fallacy is possible. Professor Flemming must be quite aware that at the time I wrote (and even still, although not to the same extent), the belief that the reduction of chloride of gold in the tissues takes place in, and indicates cell-processes was held by histologists of the first rank. I will only indicate as an example the figure of a gold preparation of the cornea by Rollett in Stricker's '*Handbuch*,' in which a similar reduction of the gold chloride is described as corresponding to the corneal cells. My scepticism on the subject was first excited whilst engaged on a study of the cornea in the summer of 1874, and in September of the same year, in a note on the corneal cells appended to a memoir in the '*Edinburgh Medical Journal*,' I expressed my conviction that the appearances produced by gold and described as cells, are nothing but a metallic deposit in the interstices of the tissues. I have frequently had occasion since to urge the same fact both as regards gold and silver preparations.¹

¹ The professor's difficulty about the perimysium in the figures 5 and 6 of the same memoir, that in one case it is applied to the surface of a fibre, and in the other goes straight over two fibres, would have been diminished if the accessory parts of the preparation had been sketched. There was nothing drawn except

Whether I am correct in describing layers of flat cells in the perimysium is another question. The activity that prevails in histological studies in so many parts of Europe, may probably be entrusted to answer it within the next few years.

Professor Flemming further decides at a glance that figures which I describe as representing cells from the cornea and cutis are the ordinary epithelial cells of these structures. As Professor Flemming does not profess ever to have seen a cell belonging to the cornea or cutis, and as it is probable that he never has—I willingly give him credit for refusing to accept the ordinary appearances described in text-books as corneal cells—his summary decision regarding the cells which I claim to have isolated seems to me a hasty one.

Waldeyer, in the memoir already quoted, describes the cell of the subcutaneous tissue as being similar to that which he found in tendon. Wherever, he remarks, there is fibrillated connective substance the essential form (*Grundform*) of the so-called fixed connective-tissue cell is neither fusiform, nor that of a rectangular plate (the older and newer views), but that of a compound system of plates (like an open book). From the periphery of the chief and subsidiary plates there is continually given off a number of fine thread-like processes. He further describes another variety of cells found isolated in the neighbourhood of blood-vessels as plasma cells. They are large round cells with coarse protoplasm.

We thus find that there is no more unanimity regarding the cells of the subcutaneous tissue than there is about the cells of tendon. If any one wishes to know from the evidence of histologists whether they are rounded, or square, or elongated bodies; whether they have clean contours or have processes; whether they are isolated or are joined so as to form a layer, he will find it impossible to come to any conclusion.

I have, from considerations of space, confined my attention to the cells; but there are other important points, regarding which opinion is equally unsettled. When, for example, a bundle of fibrillary connective tissue is made to swell by a peculiar process, it is found to be snared at certain parts. Histologists differ as to whether this snaring is produced by a fibre, or by the unequal bursting of a structureless sheath, or of a sheath which, being strengthened by fibres like the ribs of a leaf, gives way naturally at some parts before others, or both by true elastic fibres and by coagulation of a cement sub-

what was deemed essential. The one represents the appearance seen on the perimysium, which encircles every muscular fibre in the frog; the other on one of the larger tracts of connective tissue that are found enveloping a group of fibres. That both exist is too easily demonstrated to require further remark.

stance (*Kittsubstanz*) which is supposed to be on the surface of the bundles.

The cornea affords another example of the differences of opinion that prevail at present regarding the nature of cells. Rollett, in Stricker's 'Handbook,' describes the corneal cells as branching bodies anastomosing abundantly with each other by comparatively wide protoplasmic processes, a view that is also held by Professor Stricker. Schweigger-Seidel, on the other hand, an authority on such a matter second to none, in a memoir in the 'Leipzig Arbeiten' for 1869, maintained that the cornea corpuscle of Rollett is not a cellular element at all—is, in fact, not an independent structure,—but is of the same nature as the amorphous interfibrillary cement-substances. The corneal cell he held to be a thin, flat, plate-like, rectangular body, somewhat similar to the tendon cell of Ranvier. These flat bodies are isolated, or occur two or three together, in which case they are joined by processes. Waldeyer agrees with neither. His cornea cell is essentially the same as his cell in tendon and in the subcutaneous tissue. It is not the so-called protoplasmic cell of Rollett, nor is it the flat cell of Schweigger-Seidel. It has several sides and angular projections, and sends out flat ribbon-like processes.

I have ventured to give a view of the corneal cells which differs from all these authorities. I believe ('Proceedings of the Royal Society,' 1875, and 'Edinburgh Medical Journal,' November, 1875) that there are layers of flat cells in the cornea which invest the lamellæ, that the bundles themselves are invested by cells, whilst between the lamellæ there are branched cells which send out numerous fine processes. These processes are true fibres and pierce the lamellæ between the investing cells, and forming a network around the bundles bind them together. I further believe that there are chains of minute spindle cells which lie parallel to the bundles.

Every year adds its quota to the literature of the cornea, but opinions continue to diverge, and no one who is familiar with the nature of the controversy that the subject has called forth can be sanguine enough to hope that the end will be soon.

The spindle or fusiform cell, which figures so prominently in histological works of a not very old date, is interpreted by most of the authors I have quoted as being a profile view of a flattened cell. It is not believed to exist in the independent typical form suggested by the name. Whilst it is quite certain that most of the appearances described as spindle or fusiform cells are in fact nothing but the profile view of the flat cell, I have however satisfied myself that the true spindle cell does

exist. I have isolated it from the neurilemma of the sciatic nerve of the frog, and Mr. Ewart and myself have succeeded in isolating it from the internal granule layer of the retina ('Journal of Anatomy and Physiology,' October, 1876). The radial fibre of the retina, as it is figured by H. Müller and Kölliker, and by us in the same memoir, I believe to represent a phase in the development of a system of branched cells of a different order from the spindle or fusiform cell.

It would become tiresome if I continued to pile contradiction on contradiction without trying to illustrate what I believe to be the nature of the causes that underlie so much diversity of opinion. These are so numerous and complicated that I shall select only one or two as examples. First and foremost, such an absence of unanimity implies that we are still comparatively on the threshold of the subject, and that its investigation is attended with many difficulties. A study of recent histological literature has indeed convinced me that this difference of opinion begins at the A B C of the whole matter. It seems to me that the idea conveyed by the words *cell* and *protoplasm* is so elastic that, although they are constantly employed, one is never certain what exact meaning an author intends to convey when he uses them. When I speak of an epithelial cell from the surface of the cornea no one can mistake what I mean, because the nucleated element spoken of has a definite and uniform contour and size; its varieties even are uniform and limited. But when a fragment of tissue with indefinite contour and varying size, sometimes with prolongations and sometimes without them, and without any uniformity in the prolongations when they are present, has a nucleus on it or in it, am I justified in calling that mass a cell? We have seen already that although such appearances have been described as cells by histologists who receive and deserve the highest respect, no less authority than that of Key and Retzius summarily sets them aside as membranous remnants.

It may be assumed that, by common consent, a lymph corpuscle is a fair type of what is termed protoplasm; but what reason is there to believe that much of what is seen in cells that have arrived at their mature development, and which is also termed protoplasm, has anything in common with the substances so designated in the lymph corpuscles? If protoplasm means anything, it conveys the idea of life and capacity for development; but what proof is there that the granular cells sometimes found in the tissues, and said to contain, for example, coarse protoplasm, are living and capable of any vital change? For aught we know, the granular matter which makes them comparatively easily seen is a product of the decay

and death of cells which, in their normal condition, usually escape detection.

But the ambiguity of the word becomes much more serious when it is applied to substances that are not cellular at all. We read of thin layers of protoplasm in the tissues, the expression being frequently used in reference to undoubted extra-cellular substances; but will any one give a definition of protoplasm as distinct from the living substance of a cell? Is matter in the interstices of the tissues that is granular or that has a certain amount of tenacity ever to be considered as protoplasm? If so, how much of it, and how is the protoplasmic part of it to be distinguished from what is not?¹

Again, we read a great deal about *Kittsubstanz*, or cement substance. It plays a conspicuous part in the tissues, according to some histologists; but by what characters is it to be recognised? Where one histologist sees a membrane or sheath another sees cement-substance. It is clear that they do not mean the same thing. What are the characters which an unprejudiced observer is to look for who wishes to decide whether a certain appearance is a structureless sheath or cement substance? It is to be presumed that it is a formless substance. By what criterion is it to be distinguished from other formless substances?

Another source of disagreement amongst histologists is the different interpretation given by them to appearances produced by the reagents which they use in their investigations. This may be conveniently illustrated by a reference to the effects produced by nitrate of silver, and the contradictory explanations which they receive.

When fresh tissues which have been brought into contact with a weak solution of nitrate of silver are exposed to sunlight a dark precipitate of albuminate of silver becomes visible. The precipitate does not take place equally throughout, and it is this peculiarity which gives its value to the silver method. Some of the effects thus produced give rise now to no contro-

¹ Since the above was in type, I have seen the first half of the promised work (*Studien in der Anat. des Nerv. Syst.*) by Key and Retzius—a work equally honorable to the authors and the Count von Platen, by whose assistance it is published—and I find in it a passage that strikingly illustrates the point I am now discussing. The authors observed (p. 129) around the subarachnoid trabeculae a substance apparently consisting of granules in a clear medium, and remark that at first sight it might be believed that this was a coarsely granular protoplasm. On ascertaining that the apparent granules are optical sections of fine fibrillæ, they determine it to be a fibrillary sheath. We are thus left to infer that granular matter in a clear medium is equivalent to protoplasm. The observation is otherwise of especial interest to me, as I find an analogy between this fibrillary sheath and an extremely delicate substance which I have isolated in serum in the form of fine bundles from the connective tissue of the sciatic nerve of the frog ('Proc. Roy. Soc.,' 1875).

versy. On the surface of a serous membrane, for example, the reticulum of dark lines which forms can be shown to correspond to the contour of the covering cells of the membrane. It has hence been naturally inferred that, wherever a similar reticulum of silver lines can be demonstrated, there exists a layer of cells.

But there is another effect of the silver solution, regarding which there is anything but unanimity, although very important histological doctrines have been founded on it. An example of it may be easily produced in the cornea, but it can be shown with greater or less ease in other forms of connective tissue. When the solution has penetrated beyond the epithelium of the cornea the *substantia propria* of that structure is stained a varying shade of brownish-black, and in the dark ground a number of colourless figures are seen. These figures are stellate, having wide central points and a varying number of processes, which anastomose and show very distinctly against the dark ground. By modifying the conditions of the process the colourless anastomosing figures may be replaced by dark figures filled with black granules of albuminate of silver, which stand out as clearly against the feebler shade and uniform staining of the ground-substance as the colourless figures did. Now, this effect of silver solution has been interpreted in a very different manner by different observers. Von Recklinghausen, to whom we are indebted for much of the knowledge that we possess regarding the mode of application of nitrate of silver and the appearances produced by it, has taught that these white stellate figures correspond to a set of lymph canals or channels in the tissues. He has been followed in this interpretation by many histologists, who have characterised the colourless appearance as the "negative image." The corresponding appearance of black granules of albuminate of silver they believe to correspond to a system of branched cells, which lie in these channels, and they characterise it as the "positive image." But the distinction between the positive image as belonging to the cells, and the negative image as belonging to the channels, has not been always clearly adhered to; and the same author, in some passages, speaks of them as being distinct, and in others as being identical.

Von Recklinghausen's interpretation has never commanded universal acceptance. It was early met by Schweigger-Seidel with trenchant criticism, and it is incompatible with the newer views of structure taught, for example, by Ranvier. For myself, I believe that all that the so-called negative image as usually seen¹ teaches us is that parts of the ground substance recede

¹ As colourless fields can be produced on the surface of sections of fresh connective tissues, it follows that there must be a substance present there that

from each other where the colourless fields are, and that the positive image shows that there is an albuminous substance, of a peculiar nature, distributed in the clefts which are so formed. The silvered cornea does not even teach us that the apparently homogeneous ground substance is as structureless as it seems. It may be composed of separate bundles and layers, as I hold it is, and these may be in such intimate contact with each other at the time they come under our observation that they form an apparently homogeneous mass. As regards the positive image, it does not teach us whether the albuminous substance is structural or amorphous, intra-cellular or extra-cellular, or both at the same time, and is *à priori* equally compatible with the conception of a corresponding system of branched cells, lymph channels, or, as I hold, dilated intermembranous interstices.

The arguments for and against either of these interpretations must be mainly drawn from the results of other methods; and it does not fall within the scope of this paper that I should discuss them here. But, as an illustration of the great importance that must be attached to the manner in which these silver figures are read, I may direct attention to Dr. Klein's work on 'The Anatomy of the Lymphatic System; I. *The Serous Membranes*,' a treatise which is, or ought to be, within the reach of all who take an interest in this connective-tissue question, and is, perhaps, more available to English readers than any other work I could select. The author observes that "there is only one mode of preparing that gave him good results; that is, the mode of staining the fresh omentum with silver solution after having pencilled it gently on one surface," &c.; and he gives a valuable and elaborate set of plates, which show that in his hands the difficult art of manipulation with silver has attained in this instance a high degree of success. For those who interpret silver appearances as Von Recklinghausen and Dr. Klein do, the conclusions which this observer draws from his preparations can hardly be gainsaid; for those, on the other hand, who adopt a different explanation of these appearances, their bearing on histological and pathological doctrine lies in a perfectly opposite direction.

Dr. Klein's lymph canalicular system (Recklinghausen's *Saft-kanälchen*), with its corresponding branched nucleated cells, I would interpret, for example, from my point of view, as a deposit of albuminate of silver in spaces between the elements of the matrix. The nuclei seen I believe to be the

refuses the silver impregnation. I believe, for reasons explained elsewhere, that this substance is cellular. The fields on a surface indicate parts still covered by the remains of a cellular membrane, and in the depth of a tissue points where the membranes are not obscured by the stained ground substance.

nuclei of cells, the cell substance by this method not being visible. The vacuolation in the cells, which is described as going so far that a vesicle is formed, the wall of which has differentiated in endothelial plates, in my way of looking at it, simply means that in a particular part the silver deposit fails, and the outlines of previously existing flat cells become visible. These cell outlines are usually not seen; but I infer from Dr. Klein's book that their demonstration, especially in the omentum of young animals, is facilitated by chronic inflammation. In the figures which are described as "the continuation of the endothelium of the lymphatic vessels with the lymph canalicular system or, properly speaking, with their branched cells" I can only see a patch in which at one place flat cells are visible in a continuous layer like that of an epithelium, whilst between that part and a zone in which these cells are not seen there is an intermediary area in which the demonstration of flat cells has succeeded incompletely, and in which the field of view is partly composed of ground substance, and partly of fragmentary cell outlines. Where, again, for example (as in fig. 40), Dr. Klein sees a vein invaginated in a lymphatic vessel, and the lymphatic vessel in communication with the lymph canalicular system of the ground substance, I only see that, for a short distance on each side of the vein, flat cells in contact with each other are indicated, and that the colourless fields which correspond to the cells are laterally continuous with colourless patches on a coloured ground, the broken patches being crossed by lines of an exactly similar nature to those which, when in a complete reticulum on an unbroken field, are supposed to indicate an endothelium. I see no lymphatic vessel, but a demonstration of flat cells complete near the vessel, and incomplete as we remove from it. So, when further Dr. Klein uses his silver preparations in support of the doctrine that capillary blood-vessels are formed from branched cells in the tissue which anastomose with branched cells in the wall of a pre-existing vessel, the value of the inference depends on the accuracy of the view which identifies the deposit of albuminate of silver in a stellate form with stellate cells. In such preparations I, for instance, can only see the presence of a peculiar formless albuminous substance, which can be tracked from the wall of the vessel into the interstices of the tissue. With the accuracy of one special mode of interpreting the effects of nitrate of silver, which is not universally accepted, Dr. Klein's conclusions regarding the anatomy of the omentum must stand or fall.

Those who believe that the "positive image" represents a cell, and that there can be no continuous layers of flat cells in the substance of a serous membrane, must accept Dr. Klein's

teaching. Those, on the contrary, who hold different views on these points will not fail to find their way of thinking confirmed by the same preparations. Everything in this instance depends on the interpretation of silver appearances, and a better understanding on the subject is, therefore, urgently needed. It is fortunate that a record of work like that contained in Dr. Klein's treatise has a value that cannot be impaired, whatever theoretical explanation may be given to the appearances which are described and figured. The new facts brought to light will remain, independently of any particular interpretation, and it is certain that the book will, for some time to come, be a valuable work of reference to any one who undertakes an investigation on the anatomy of the omentum.

The same mode of illustration might be applied to some of the effects of chloride of gold, and to the effects of some simple dyes. Ranvier has, for example, described, in the omentum of young rabbits, a special kind of cell, which finally becomes a capillary blood-vessel. But the value of his conclusions depends to a large extent on whether a set of narrow, branching, band-like appearances in the substance of a tissue which stain in carmine, which have never been isolated, in which, although nuclei are visible, there is neither uniformity in the position of the nuclei, nor in the outline of the carmine-stained appearance, can be considered as a cell.

But if the foundations of the normal histology are unsettled, how does it fare with pathological histology? On many points there is naturally the greatest confusion. We have seen that there is no unanimity as to what constitutes the cell in connective tissue, and yet it is not uncommon to find pathologists discussing the behaviour of cells in inflammation, whilst they leave the cell itself undefined. They tell us that they have observed cell proliferation, but they frequently forget to tell us what the nature of the thing is which proliferates, whether, for example, it is the cell of Virchow, or of Ranvier, or of Waldeyer. And yet, until a clear idea is obtained of what the normal cell is, how is it possible to follow its pathological changes? Most of those who do not accept the "connective-tissue corpuscle" of Virchow as the cell of the cutis—and, probably, not many histologists now do—must believe that we know nothing certain about the cells in that structure. How, then, follow changes that take place in these cells in such processes as cancer and lupus? The same kind of questions might be carried much further, but for the purpose of illustration I have probably said enough.

The difficulties I have raised will have probably suggested to some readers the comfort and necessity of falling back on

the "authorities" in such obscure matters. But I fear even this source of consolation cannot be rationally enjoyed. If anywhere, it is in science that Victor Hugo's political utopia in which the authorities shall be *les auteurs*, the doers, already exists. But it is precisely in histology that the doers counteract each other. It is the doers who adhere with most tenacity to the most contradictory opinions.

There is unfortunately, no doubt, then, that dogmatic teaching, as regards many vital points in histology, is still impossible. But whilst I have purposely brought into prominence the subjects on which histologists differ from each other, it would be misleading if I were to close without testifying to the fact that a steady progress is constantly taking place. It is not rapid, and steps which are apparently in the right direction must often be retraced. But there is an advance, notwithstanding. The road is full of pitfalls, and only those avoid them who stand aside and content themselves with watching what is being done; but, in spite of the difficulties produced by a still very imperfect *technique*, new facts are ever and again conquered, and even mistakes sometimes prove a source of unexpected acquisitions.

Chronicle of Medical Science.

REPORT ON PHYSIOLOGY AND HISTOLOGY.

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CONNECTIVE TISSUE—BLOOD.

1. THOMAS E. SATTERTHWAITE. *On the Structure and Development of Connective Substances.* In the 'Monthly Microscopical Journal,' Oct. 1 and Nov. 1, 1876.
2. Dr. L. SCHÖNEY. *On the Ossification Process in Birds, and the new formation of red Blood-corpuscles during the Ossification Process.* In the 'Monthly Microscopical Journal,' August 1, 1876.
3. Dr. EDUARD EIDAM. *Untersuchungen über Baeteriea.* In 'Cohn's Beiträge zur Biologie der Pflanzen,' heft iii, 1875.
4. L. RANVIER. *Recherches sur les éléments du sang.* 'Archives de Physiol.,' &c., 1875, p. 1.
5. J. TARCHANOFF et A. SWAEN. *Des globules blancs dans le sang des vaisseaux de la rate.* 'Arch. de Physiol. Normale,' &c., 1876, s. 324.
6. J. TARCHANOFF. *Note sur l'effet de l'électrisation du sang des têtards sur le mouvement des granulations vitellines contenues dans les globules rouges.* (Travail fait au Laboratoire d'Histologie du Collège de France.) 'Arch. de Physiol.,' 1875, s. 756.
7. TILLMANNS. *Die Lymphgefäße der Gelenke.* 'Archiv für Mikroskop. Anat.,' xii, p. 649.
8. M. ABELES. *Der physiologische Zuckergehalt des Blutes.* In 'Wien. Med. Jabrb.,' 1875, p. 269.

1. Dr. SATTERTHWAITE describes mucous tissue, as seen in the umbilical cord of the embryo, fibrous tissue from the same source and from the mesentery of the frog, and adenoid tissue. He describes the branching bundles of fibres observed in the two latter cases as proceeding from changes in the intercellular substance. He describes neuroglia as a non-anastomosing interlacement of delicate fibres. He also briefly describes tendon, fat, intermuscular tissue, connective tissue of the kidney, and corneal tissue.

2. Dr. Schöney holds with Heinrich Müller and C. Heitzmann that cartilage is not connected directly with bone tissue, but that

marrow formation precedes bone formation. This, however, is only true of young animals; in older ones (birds) the ready made bone directly joins the cartilage, and in fully grown pigeons the marrow spaces of the bone forms prolongations which extend into the cartilage to a certain height. He confirms Gegenbauer's view that the flat protoplasm bodies called osteoblasts are real bone formers. The osteoblasts, he finds, are connected with each other by delicate threads. In regard to the formation of red corpuscles of blood from the ossifying border, Schöney supports Heitzmann and Aeby in maintaining that they do arise in this way.

3. Dr. Eidam, in the first of a series of papers on the biology of Bacteria, gives the results of his experiments on the effects of various temperatures and of desiccation on the development of *Bacterium termo*. He finds—1st. That *Bacterium termo* passes into a state of cold-tetanus at $5^{\circ}\text{C}.$; at $5.5^{\circ}\text{C}.$ it begins to undergo multiplication, which, however, only progresses very slowly. 2nd. Temperatures between 30° to $35^{\circ}\text{C}.$ (86° to 95°Fahr.) are the most favorable for its rapid development. 3rd. When *Bacterium termo* is continuously exposed to a temperature of $40^{\circ}\text{C}.$ or more, its power of propagation ceases; it passes into the condition of heat-tetanus, from which it awakens as soon as the conditions are favorable. 4th. Exposure to a temperature of $45^{\circ}\text{C}.$ (113°Fahr.) for fourteen hours continuously, or for three hours at $50^{\circ}\text{C}.$, is sufficient to kill *Bacterium termo* equally distributed in a nutritious fluid. 5th. The disintegrating process usually termed putrefaction is induced or occasioned by the life-activity of *Bacterium termo* with the same certainty as alcoholic fermentation by the presence of the various species of saccharo-mycetes. Rapid increase of *Bacterium termo* always characterises living putrefaction, and, *vice versâ*, the latter ceases as soon as too high or too low a temperature has caused the *Bacterium termo* to cease to increase. 6th. As *Bacterium termo* must be regarded as the proper produce of putrefaction in dead organic matter, other forms of Bacteria appear to excite other forms of decomposition, which differ from one another considerably according to the species of Bacterium developed. 7th. *Bacterium termo* possesses the power of resisting and preserving its vitality after long periods of desiccation.

4. Ranvier addresses himself first to the nucleoli contained in the nuclei of the red corpuscles of the amphibia. The nuclei of the blood-corpuscles of the frog, axolotl and proteus contain, he observes, under normal conditions sharply defined and characteristic nucleoli. In the frog there is usually only one, in the axolotl and especially in the proteus two or three are not unfrequently found. In fresh blood the nucleoli can only be seen with great difficulty, but an unfailing method of exhibiting them consists in adding to a drop of blood on a slide two or three drops of a mixture of one part of alcohol and two parts of water. Secondly, he discusses the membrane of the red blood-corpuscles. To bring this into view he treats a few drops of frog's blood, with diluted alcohol and a solution of sulphate of rosanilin in alcohol. The hæmoglobin exudes into

the plasma ; the corpuscles appear clear and transparent, their nuclei of red colour and granulated. A distinct doubly contoured membrane is rendered apparent by the colouring at the periphery of the corpuscles. During this process the blood-corpuscles lose their normal shape. Various phenomena, which can easily be perceived by direct microscopic observation, demonstrate that this membrane consists of a very soft and yielding substance ; it permits solid corpuscles to pass through it, and closes again without leaving any trace. Thirdly, in regard to the nuclei of the colourless blood-corpuscles, he finds that only the smaller ones possess spherical nuclei ; the nuclei of all the larger ones are very irregular in form. Lastly, he observes that increase of the colourless blood-corpuscles by fission, as described by Klein, can most easily be observed in the leucocytes of the axolotl, which are larger and more transparent than those of other animals. Ranvier gives the details of such a division, which occupied three hours and twenty minutes. During the whole period the protoplasm exhibited the liveliest amœboid movements, whilst the nucleus appeared to be passive.

5. According to Tarchanoff and Swaen, the great abundance of colourless blood-corpuscles generally stated to exist in the normal venous blood of the spleen is incorrect. The blood of the splenic vein almost always contains fewer white corpuscles than that of the splenic arteries ; in very few instances could a small excess be demonstrated. They found that all circumstances which led to enlargement of the spleen were followed immediately by a diminution in the number of blood-corpuscles in the splenic venous blood ; this was particularly remarkable in that enlargement of the spleen which followed section of the splanchnic nerves. If the blood of various vascular provinces of the body were examined during the enlargement of the spleen consequent upon such section of the splanchnic nerves, the blood was found to be progressively poorer in white corpuscles until a kind of equipoise had become established between the splenic veins and arteries in this respect. This impoverishment of the blood in white corpuscles, they point out, is capable of being explained in three ways first, by a mechanical accumulation of the blood-corpuscles in the spleen ; secondly, by their destruction in the interior of the spleen and thirdly ; by their conversion into red blood-corpuscles. The experiments of Malassez and Picard chiefly give countenance to the latter view.

6. Tarchanoff found that the red blood-corpuscles of young (two to four weeks old) tadpoles submitted to induction current, exhibited Porret's phenomenon. The blood-corpuscles of these animals contained yolk-like granulations, which under normal conditions exhibited molecular movements. Under the influence of an electrical current all the granulations accumulated at one pole of each blood-corpuscle ; if the direction of the current was reversed they accumulated at the opposite pole. When the electrical current was intermitted the granulations again distributed themselves equally through the substance of the blood-corpuscles. Tarchanoff concludes from

this that in young tadpoles the matrix of the red corpuscles is fluid and offers no opposition to the movements of the granulations.

7. Tillmanns in the first instance adopted Ludwig's suggestion to inject the lymphatics of the joint by throwing in colouring matter in solution into the knee-joint of a freshly killed dog, and then flexing and extending the extremity. It was found, however, that although this method was successful in the case of tendons and fasciæ, it was useless in the case of synovial membranes. In subsequent experiments Tillmanns readily effected his object by puncturing the synovial membranes of oxen and horses and injecting a half per cent. solution of silver. He found that the most superficial lymphatics of the synovial membranes lie immediately beneath the endothelial membrane, and also beneath the finest blood-capillaries, which are certainly covered by an epithelium. He was unable to demonstrate the presence of lymphatics in the villous processes of the joints. The most superficial sub-endothelial lymph-paths become at once wide vessels as they pass into the deeper connective tissue; here they are very numerous and not unfrequently surround the blood-vessels. The presence of the lymphatics is most easily shown by punctures made at the point of attachments of the synovial membrane to the bone. No lymphatics penetrate from the synovial membrane into the bones or cartilage.

8. M. Abeles found that sugar was present in the blood of all regions of the body of the dog in not inconsiderable quantity, averaging 0.05 per cent. He showed that this existed in the form of grape sugar by its action in reducing copper oxide and bismuth; by fermentation, by its circular polarisation to the right, and by the properties of its combination with alkalis. The arterial blood contained on the average 0.047 and the venous 0.053 per cent. of sugar. The blood of the right heart and of the vena cava just after the junction with it of the hepatic vein, showed no perceptible difference in the amount of sugar they contained, which was opposed to the view that it was derived from the liver.

DIGESTION.

1. DR. L. VON THANHOFFER. *Histologische Mittheilungen*. In 'Centralblatt für die Med. Wiss.,' No. 23, 1876.
2. R. KLEMENSIEWICZ. *Ueber den Secus Pyloricus*. 'Sitzungsber. d. Wien. Acad.,' lxxi, 3, Märzheft.
3. PROFESSOR RUTHERFORD and M. VIGNAL. *Experiments on the Biliary Secretion of the Dog*. 'Journal of Anatomy and Physiology,' vol. xi, p. 61.
4. BULGAK. *Ueber die Contractionen und die Innervation der Milz*. In the 'Centralblatt f. d. Med. Wiss.,' 1876, p. 577.
5. CL. BERNARD. *Critique expérimentale sur la Glycérine*. 'Révue Scientifique,' July 1st, 1876.
6. N. KONJKOFF. *Ueber den Einfluss gewisser Agentien auf die Menge des Glycogens der Leber*. Diss Petersburg, 1876, p. 36; ref in 'Petersb. Med. Wochenschr.,' 1876, No. 4.

1. Dr. von Thanhoffler states that the villi of the small intestine, impregnated with a two per cent. solution of nitrate of silver, are separated from the epithelial cells by a basal membrane containing nuclei, which, as Debove maintains, is probably formed of flat endothelial cells. 2. The intercommunicating network of juice canals is visible in the basal substance of the villi. 3. These juice networks form a canalicular system, the tubes of which are provided with membrane. The nodal points of this canalicular system are termed by Waldeyer connective-tissue-cells. The nodes contain bright round or oval nuclei, surrounded by a little spherical or radiating mass of protoplasm. These nuclei represent the nuclei of flat connective-tissue-cells or endothelial cells. 5. The stellate juice- or fat-canals are in direct continuity with the sheaves of the epithelial cells covering the villi, and in the process of absorption receive the fat-globules from the latter. 6. These canals, into which the fat first enters, are directly continuous with the lacteals in the middle of the villi. 7. The central lacteal is not a space destitute of walls, but is a canal formed by an epithelial cell membrane. 8. The path traversed by the fat-globules stands also in direct communication with the blood capillaries. 9. Part of the so-called nuclei or cells of the stroma are the nuclei of large flat epithelial-like cells which line the reticular spaces of the passages traversed by the fat. Villi macerated in perosmic acid (1 per cent.) or nitrate of silver, from which the epithelial covering has been detached, exhibit Brücke's muscle elements and other muscular fibres arranged in a circular manner. 11. The centre by means of which movements are excited in the rod-like processes of the protoplasm of the epithelial cells of the villi is situated in the medulla oblongata, in the *rana esculenta*, in the middle line of the upper part of the sinus rhomboidalis.

2. Klemensiewicz established fistulae in the pyloric portion of the stomach, in order to settle the much disputed question whether this part of the stomach really secretes a digestive fluid. The first researches were performed on the method adopted by Thiry, of isolating particular portions of the intestine; thus, the pyloric portion was isolated, opened, and the margins of the fistula were united with the edges of the wound of the skin, the duodenum and fundus of the stomach being at the same time united by suture. The operation succeeded well, the cardiac portion of the stomach and the duodenum uniting perfectly, though the animal always died in a few days from peritonitis. It was necessary, therefore, to confine the observations to the secretion obtained during the first few days. It was tenacious, gelatinous, yellowish, in thin layers transparent, in thick layers cloudy, and of distinctly alkaline reaction; the specific gravity varied from 1.01 to 1.09; the amount of solids varied from 1.65 to 2.049 per cent.; it contained a little albumen. The secretion, when its reaction was normal, had no action upon fibre, but dissolved it as well as boiled white of egg with facility when acidified. In addition, the secretion dissolved the collagenous substance of tendons and converted starch into sugar. The digestive action was thus shown to be at least as active, and often was even more

powerful than that of the secretion of the fundus, which was in some later experiments coincidentally obtained from a fistula made at the cardiac extremity. The ferment of the pyloric extremity contains pepsine, and is even richer in it than the secretion of the cardiac extremity. The pyloric portion of the stomach is recognised by its paler colour, and extends, in small dogs, for a distance of from five to six centimètres from the pylorus. The so-called peptic glands are entirely absent from the pylorus.

3. The paper of Professor Rutherford and M. Vignal is in continuation of their former experiments. The animals experimented on were dogs. These had always a full meal of flesh at four o'clock in the afternoon, and the experiment was commenced at nine o'clock on the following morning. A small dose of curara was given, artificial respiration maintained, a glass canula tied in the common bile-duct, the cystic duct clamped, and the amount of bile collected recorded every fifteen minutes. The various substances were injected directly into the duodenum. The substances used were euonymin, sanguinarin, iridin, leptandrin, ipecacuan, colocynth, and jalap. The results of the experiments with the three last, which are the most important, are as follows:—Sixty grains of powdered ipecacuan, mixed with a small quantity of bile and placed in the duodenum, powerfully stimulated the liver. The bile secreted under its influence was of normal composition. No purgative effect was produced, but there was increased secretion of mucus in the small intestine. It is, therefore, certain that this substance has the power of stimulating the secreting apparatus of the liver. The results of the experiments with colocynth were that colocynth is an hepatic stimulant of considerable power; it renders the bile more watery, but, nevertheless, increases the secretion of biliary matter. It is also a powerful stimulant of the intestinal glands. The results of the experiments with jalap showed that jalap is also an hepatic stimulant of considerable power; it renders the bile more watery, but, at the same time, increases the secretion of the biliary matter; its effect on the liver is, however, far less notable than its effects on the intestinal gland; its hydragogue cathartic effects on these were fully manifested in these experiments.

4. Bulgak's experiments on the contraction and innervation of the spleen were undertaken on dogs, previously partially narcotised either by the intravenous injection of morphia or the administration of chloroform. A crucial incision was made in the abdomen, and, to avoid the effects of mere exposure and drying, the parts were covered with soft linen steeped in artificial serum raised to a temperature of 98° F. The stimulus was in all instances a weak induced current. The results obtained have satisfied him that the nerves surrounding the splenic vessels are of two kinds—centripetal and centrifugal. Excitation of the central extremity of a divided centripetal nerve induces well-marked general contraction of the spleen, with pain, indicated by the movements of the animal. No such results follow the excitation of the distal extremity. On the other hand, excitation of the central end of a centrifugal nerve causes no

obvious change in the spleen, whilst excitation of the peripheric end induces a local contraction, limited to the part to which the nerve is distributed. The simple act of section of a centripetal nerve causes no change in the spleen, but division of a centrifugal nerve occasions circumscribed local swelling and blueness of the organ. If the electrodes are placed in direct contact with the spleen, the tissue between them exhibits contraction. When local or general contraction of the spleen is induced, the mode or order in which contraction takes place is as follows:—The surface of the organ first becomes paler than natural and finely granular. It then becomes altogether smaller, harder, and slate-grey in colour. After the cessation (*Einstellung*) of the stimulus it first becomes rosy and then smooth on the surface, returning gradually to its normal state. In order to determine whether the changes observed on nerve irritation produced any effect on the number of white corpuscles arising from the spleen, he took specimens of blood from the splenic veins, mixed them with weak solution of common salt, and counted the white corpuscles present. The results obtained showed that the number of white corpuscles issuing from the spleen was at its maximum in the ordinary non-contracted condition of the spleen, that it diminished on contraction of the organ, and attained its minimum during the subsequent swelling. If, however, the spleen was made to contract after it had been previously swollen by ligature of the veins, the number of leucocytes was greatly augmented. Bulgak found that the contractions of the spleen were affected by various circumstances. They were weakened by curara and protracted narcosis, the spleen ultimately becoming soft and blue, and incapable of any contraction. (2) The injection of quinine into the veins caused contraction of the spleen and increase of the white corpuscles in the venous splenic blood. (3) Ergot caused no contraction, even in large doses. (4) Asphyxia, and all conditions interfering gradually with the exchange of gases in the blood, cause strong general contraction of the spleen. (5) Irritation of the peripheric end of the vago-sympathetic nerve in the cervical region causes no contraction; but irritation of the centric end produces strong contraction, coincidently with spasm of the diaphragm and arrest of respiration. Still stronger contraction of the spleen follows excitation of the central end of the laryngeal nerve by strong currents, which also, as is well known, cause arrest of respiration in the condition of expiration or relaxation of the diaphragm. Both results are due to interference with the interchange of gases in the blood. (6) Excitation of the semilunar ganglion causes in dogs and rabbits well-marked general contraction of the spleen. (7) Atmospheric air alone exerts no influence on the spleen if cooling and evaporation through its means be prevented. As general or local contraction of the spleen is excited by irritation of the centric extremity of divided centripetal nerves, it is evidently of a reflex nature; but since it is always arrested by division of the splanchnics above their entrance into the coeliac ganglion, this ganglion is certainly not the centre, which must therefore be looked for in

the cranio-spinal axis. Experiments made to determine its position led to the following results:—Excitation of the spinal cord at the level of the membrana obturator atlantis posterior never caused the slightest contraction of the spleen; but excitation applied to the cord opposite the atlas immediately induced well-marked contraction. So, again, if the cord were divided just below the medulla oblongata, contraction could readily be obtained (the animal being kept alive by artificial respiration) by excitation of the central end of centripetal splenic nerves. Strong contraction was called forth by excitation of the cord as low as the fourth cervical vertebra; below this the effects produced were progressively more feeble to the eleventh dorsal vertebra, below which they ceased altogether. From this he concludes that the splenic centripetal and centrifugal nerves enter the cord at the level of the eleventh dorsal vertebra, and ascend to a centre situated in the cord between the level of the fourth and first cervical vertebrae. From this reflex and motor splenic centre the nerves to the muscular tissue of the spleen itself proceed, and it is probable also that the splenic vaso-motor nerves come from the same source. Bulgak lastly endeavoured to ascertain through what channel the centrifugal and centripetal nerves of the spleen proceeded, and satisfied himself by section that they ran in the left great splanchnic nerve; irritation of the distal portion of this nerve when divided causing well-marked and persistent contraction of the spleen, and that of the proximal end shrieking of the animal, whilst no contraction was produced by irritation of the other splanchnics, nor of the vagus in the neck.

5. M. Bernard, in a note read before the Academy of Sciences, states that from recent experiments he has satisfied himself of the accuracy of the three following facts:—(1) That the sugar of the blood undergoes rapid destruction after the withdrawal of the blood from the body. (2) It also undergoes rapid destruction after death, outside the vessels. (3) The amount of sugar contained in the blood of a living animal is subject to constant variation. He finds that sugar is always present in the blood, and that glycæmia is a constant phenomenon of the living organism, whilst it is arrested after death.

6. Konjkoﬀ arrives at the following conclusions:—(1) In the normal liver taken from a living rabbit no sugar, or only traces of sugar, can be discovered. (2) All glycogen disappears from the liver after four days' complete fasting, supposing the rabbit to have previously been normally fed. (3) The injection of cane or grape sugar into the stomach of a rabbit causes a considerable accumulation of glycogen in the liver in the course of six or eight hours. (4) This does not occur if mannite be given. (5) Arsenious acid mingled with the food, when given in large and poisonous doses, causes complete disappearance of the glycogen, and in small doses a considerable diminution of its quantity. (6) If arsenic and sugar be given together there is no accumulation of glycogen. (7) Nitrite of amyl and nitro-benzine act in the same manner as arsenic in causing the disappearance of glycogen. (8) If grape sugar and

nitrite of amyl be given together there is no increase of glycogen in the liver. (9) The elimination of sugar caused by amyl-nitrite appears to be associated with this fact.

RESPIRATION—ANIMAL HEAT.

1. KÜTTNER.—*Studien über das Lungenepithel*. 'Virchow's Archiv,' lvi., s. 12.
2. Prof. Dr. EULENBURG and LANDOIS. *Ueber thermische von den Grosshirnhemisphären ausgehende Einflüsse (vaso-motorische Apparate der Grosshirnrinde)*. 'Centralblatt für die Med. Wiss.,' p. 260, 1876.
3. Prof. E. HITZIG, in Zürich. *Ueber Erwärmung der Extremitäten nach. Grosshirnverletzungen*. 'Centralblatt für die Wiss.,' No. 18, 1876.

1. According to Küttner, the normal alveoli of the lungs both during fetal life and after birth possess a lining of cells which are in direct continuity with those of the remainder of the bronchial tree; in fact, that the lungs have no special form of epithelium, all forms of epithelium being represented in it, the particular part where they exist determining their form and size; the cervical cells of the embryonal alveoli, without undergoing fatty degeneration, becoming converted during the first inspirations into pavement epithelium.

2. The experiments of Eulenburg and Landois were made upon young dogs, and the temperatures were taken by thermo-electrical apparatus. The animal, deeply chloroformed, was trephined, the roof of the skull removed, the surface of the brain exposed and destroyed to the depth of one or one and a half mm. with a red-hot copper wire. In other instances the animal was curarised, and artificial respiration was maintained; the results were as follows:—
 1. Destruction of certain parts of the cortex of the hemispheres of the brain in the parietal region produced immediate and considerable elevation of temperature in both extremities of the opposite side. The rise amounted to from $1\cdot5^{\circ}$ to 7° C., and was sometimes most marked in the fore leg, sometimes in the hind leg, appearing to vary with the position, extent, and depth of the injury. 2. The section of the cortex, which, when destroyed, produced these effects, was tolerably well defined by the sulcus cruciatus; it embraced especially the posterior and lateral part of a hook-shaped gyrus which appeared to correspond to the anterior central convolution of man and apes—the gyrus post-frontalis of Owen. The regions affecting the fore and hind limb are distinguishable, the former lying somewhat more anterior and external. 3. On the animals awakening from the narcosis, disturbances of the motility and of the muscles consciousness of the limbs on the side opposite to the injury were observed. 4. The increase of temperature was in almost all cases persistent, though with considerable variations, for a long period. The temperature usually showed signs of returning to the normal at the end of the second or third day, but was sometimes almost as high as

immediately after the operation at the end of the third week. The disturbances of motility usually gradually diminish in the course of a few days. 5. Localised electrical excitation of this cortical region, providing sufficient currents are employed, produce a slight and transitory cooling of the extremities of the opposite side, amounting to about half a degree centigrade. These results the authors consider to be due to the presence of a vaso-motor apparatus situated in the cortex of the brain, and believe it to be the terminus of the central endings of the vascular nerves situated in the pedunculus cerebri.

3. Hitzig states, in commenting upon the above paper, that he had already, a year and a half previously, observed unilateral heating of the body after extirpation of the brain, which was sufficiently marked to be distinctly perceptible to the hand.

MUSCLE—NERVE.

1. H. P. BOWDITCH. *On the Force of Ciliary Motion*. In the 'Boston Medical and Surgical Journal,' Aug. 10, 1876, and 'London Medical Record,' Oct. 16.
2. J. STEINER. *Über die Wärmeentwicklung bei Wiederausdehnung des Muskels*. 'Pflüger's Archiv,' xi, s. 196.
3. *Beobachtungen über den Blutstrom im Muskel. Vorläufige Mittheilung von W. H. Gaskell, in Cambridge*. 'Centralblatt für die Med. Wiss.,' No. 32, 1876.
4. BOCHÉFONTAINE ET COUTY. *Influence de l'Oxyde de Carbone sur la durée de la contractilité musculaire*. 'Gaz. Méd.,' 1875, No. 50.
5. Prof. RUTHERFORD. *Note on the Action of the Internal Inter-costal Muscles*. In 'Journal of Anatomy and Physiology,' vol. x, p. 608.
6. E. FISCHER. *Ueber die Endigung der Nerven in Quergestreiften Muskeln der Wirbelthiere*. In 'Centralblatt für die Med. Wiss.,' No. 20, 1876.
7. J. ELISCHER. *Beiträge zur Feineren Anatomie des Muskelfasern des Uterus*. In 'Archiv. f. Gynæcol,' B. ix, p. 10.
8. M. LÖWIT. *Die Nerven der Glatten Musculatur*. 'Wien. Acad. Sitz-ber.' B. lxxi, 1875, Abtheil 3.
9. C. SACHS. *Die Nerven der Sehnen*. 'Reichert's et Du Bois' Archiv,' 1875, S. 402.
10. K. GONIAEW. *Die Nerven des Nahrungschlauches. Eine Histologische Studie* (Mitgetheilt von Prof. Arnstein in Kasan). 'Archiv f. Mitn. Anat.,' xi, s. 479.
11. C. A. CARL. *Der Beitrag zur Frage, Enthält die Chorda Tympani Geschmacks Fasern?* 'Archiv f. Ohrenheilk.,' x, s. 152.

1. The force of ciliary motion was shown to be not inconsiderable by Jeffries Voyman, and Dr. H. P. Bowditch adopted a modification of his experiments, constructing an apparatus of a piece of thin board with a narrow strip fastened at the middle of one edge. To the

latter a strip of glass was cemented, thus projecting over the surface of the board parallel to and seven millimètres from it. A frog was so arranged as to lie upon the board with the body under the strip of glass, and the ciliated membrane from the anterior edge of the palate to the stomach, smoothly stretched over it and accessible to observation. A vehicle to be moved by the cilia was made by cementing a small oval piece of glass of 1.437 square centimètre area to a thin piece of wood of the same size. The glass surface was covered with a piece of frog's skin with the inner surface outward. This vehicle, when placed with the skin downwards upon the ciliated membrane, was carried along towards the stomach. The work done by the cilia could be increased either by placing weights upon the vehicle or by inclining the whole apparatus. It was found that the greatest amount of work was obtained with the heaviest load. Thus, the cilia, when compelled to carry a weight of 20.534 grammes up a grade of one in ten, performed in one minute for each square centimètre of surface an amount of work equal to 6.805 grammillimètres.

Looking at the subject from another point of view, Dr. Bowditch calculates that the ciliated cells perform in one minute an amount of work equal to lifting their own weight to the height of 4.253 millimètres. Schiff estimates that the heart does in one minute an amount of work equal to lifting its own weight to the height of 150 mètres, a value more than thirty-five times as great as that above given for the ciliate epithelium.

2. Steiner observes that it is difficult to demonstrate the development of heat in the re-extension of muscle after contraction, because it is not possible to eliminate the amount of heat which is produced by the contraction of the unweighted muscle. The total amount of heat must always be measured which is set free during the period coincident with the process of contraction and the subsequent extension. Steiner demonstrates that a development of heat really does take place during the re-extension of the muscle after contraction, for he shows that the total amount of heat produced is different when the muscle is weighted at the acme of its contraction, and that it increases with increasing weighting. When during the process of re-extension a positive effect is produced by such actions upon the muscle, as it is well known influence the intensity of the development of heat or the activity of the chemical processes, the conclusion may justly be drawn that such heat-producing processes take place during re-extension.

3. Mr. Gaskell, in a provisional communication to the 'Centralblatt,' states that, independently of the sudden augmentation of blood-pressure which occurs at the commencement of tetanus, and is the consequence of the pressure of the muscle upon the veins, there is also a great increase in the quantity of fluid flowing from them in a given time during and after the tetanus. Section of the nerves constantly produces an increase of the outflow of blood. Microscopic observations made upon the mylo-hyoid of the curarised frog, which was found to be very convenient for the purpose, showed

the following points:—1st. Section of the nerves always causes dilatation of the arteries with a quicker current of blood through them. The maximum of dilatation was attained in the course of from twenty to thirty seconds, and was followed by gradual contraction, so that they returned to their normal size in a few minutes. During the first nine or ten seconds after the section no change was observable in the width of the vessels, nor was any muscular contraction visible (latent period). 2nd. Chemical and mechanical excitations had a similar effect, though it is usually of shorter duration. 3rd. Excitation of the nerves by the interrupted electric current, whether strong or weak, and applied for a longer or shorter time, induced well-marked dilatation of the vessels and acceleration of the current, preceded in all instances, however, by a latent period. During the dilatation the vessel is seen to be filled with blood-corpuscles, notwithstanding the rapidity of the current. 4th. The dilatation could not be maintained for an indefinite period, contraction commencing even while the current was passing, and the vessel after the cessation of the current diminishing below the normal, and gradually returning to its original size. 5th. Two or three induction strokes with intervals of five seconds occasioned very similar dilatations. 6th. When the blood-current in the muscle had almost ceased and the vessels were nearly empty, irritation caused by an induction current so slight as to be incapable of causing muscular contraction occasioned an acceleration of the blood-current and repletion of the previously empty capillaries. 7th. In an apparently empty artery, which therefore probably only contained serum, well-marked dilatation was often observed to occur for some seconds before the appearance of the blood-corpuscles. 8th. When the animal was only slightly poisoned with curara, so that slight nerve irritation caused tetanus, a forward movement of the blood in the veins occurred, immediately followed by complete arrest of the blood-current, or even a backward current, whilst in the arteries a kind of shock was observed followed instantly by increased rapidity of the current.

4. Messrs. Bochefontaine and Couty, in their researches on the duration of the contractility of muscular fibres in animals killed by carbonic oxide asphyxia, section of the medulla oblongata and inhibition of the circulation, found that it was preserved for the longest time in animals poisoned by carbonic oxide. Now, since, according to Bert, the blood in asphyxia contains only one per cent. of oxygen, whilst the blood in animals poisoned by carbonic oxide contains according to Claude Bernard from three to four per cent. of oxygen, the result above obtained could not be referred to the more slow or less complete internal processes of oxidation occurring in the latter forms of poisoning. The authors in their explanation endeavour to show that the dead muscle does not breathe exclusively at the cost of the oxygen already contained in the blood, but chiefly at the cost of that contained in the surrounding air. This process of respiration is not performed so energetically by the poisoned muscle, with its, for this purpose, unusable blood, as by

the healthy muscle, and hence the integrity of its constituents is preserved for a longer period.

5. Professor Rutherford, after pointing out that Hamberger's well-known model of parallel bars and elastic bands is by no means to be regarded as illustrative of the action of the intercostal muscles, since the ribs, unlike the bars, are arcs which can rotate around an axis directed from the posterior ends of the ribs to the costal cartilages, has adopted the plan of applying the elastic bands to the ribs themselves, and then found that, when placed in the position of the internal intercostals, the effects of the contraction of the bands was to cause elevation of the ribs. Owing to the circumstance that the second, third, and fourth ribs are not connected one with another, they can by such bands be raised individually. When, however, a hand is placed between the fourth and fifth ribs there is a simultaneous elevation of the fifth and four succeeding ribs, owing to the junction of their cartilages one with another. When the bands were placed in the position of fibres of the external intercostal muscles, the costal elevation and consequent lateral expansion of the chest was very well marked. These experiments entirely put an end to the statement founded on Hamberger's model, that the interosseous parts of the internal intercostal muscles must depress the ribs because that is the only costal position which admits of a shortening of these fibres. Different results are obtained if the elastic bands be placed only between say the second and third or between the third and fourth ribs. The upper rib is then drawn down, while the lower is drawn up, both when the bands are arranged after the manner of the internal and of the external intercostals; and if the lower rib of the pair be held immobile the only movement is a drawing down of the upper one. Both intercostals are therefore able to elevate the ribs when the more fixed point is above, and to depress them when the more fixed point is below. When elastic bands were placed in the position of external intercostal muscles between the eight upper ribs on *both* sides of the chest, great lateral expansion of the chest resulted; when in the position of the internal intercostals, there was neither elevation nor depression of the upper end of the sternum, while the ensiform cartilage was lifted forwards to the extent of three quarters of an inch.

6. Fischer has made a series of researches on the mode of termination of the motor nerves in the muscles of all classes of vertebrata, and finds that they always terminate in end plates in mammals, birds, and reptiles. In amphibia the system of terminal fibres exist, which was discovered by Kühne. These fibres end in swellings of various forms, and never break up into finer fibrils. He was unable to satisfy himself of the mode of termination in fishes. The essential point in regard to the ending of the motor nerves is that the end plate is to be regarded as an expansion of the axis cylinder, which is in immediate contact with the contracted substance.

7. J. Elischer considers that the structure of the smooth muscle-cells is by no means homogeneous and simple, but that they are composed of laminae, and that the nerves do not terminate in a

plexus, but directly in the nuclei of the muscle-cells, where they can be recognised in the form of extremely small knots or terminal bulbs.

8. Löwit's researches were made on the bladder of *Amphibia* by means of the gold method. He finds that the fibres about to supply a fasciculus run parallel with it. Each series or row of muscular fibre-cells has a special terminal nerve fibril. The tissue of the nerve and muscle come into immediate contiguity with each other, usually near the nucleus, which part must consequently be regarded as the physiologically most important part of the muscle cell in regard to its innervation. The nerve fibril can, however, never be traced into the nucleus, but only into the muscle substance near the nucleus, and there does not appear to be either any peculiar nerve ending, nor, on the other hand, any direct continuity of structure between the nerve and muscle.

9. Sachs describes the course of nerve fibres in some of the tendons of the frog, salamander, lizard, and mouse. In the mouse the nerves do not extend beyond a very short distance from the muscular fibres. They either break up with a fine barb of non-medullated fibres or form a brush of fine pale nucleated fibres, or they may also terminate in a solid granule which Sachs named a terminal tendon knob.

10. Goniaew's observations were first made upon the ganglia of the intestinal tube. He has demonstrated that an interchange of fibres takes place between the plexus myentericus of Auerbach and the plexus submucosus of Meissner, and that these two plexus must consequently be regarded as constituting a physiological whole, the parts of which are only accidentally separated from one another; in their histological characters, the two plexuses are identical. The relation of the nerve fibres to the ganglion cells he describes in somewhat different terms from Gerlach. According to him the nerve fibres, the processes of the cells radiating in various directions, become nerve fibres by division and thinning, whilst others proceed from the single process of a unipolar cell, and run only in one direction. A second set of observations were made upon the mode of termination of the nerves in the œsophagus and in the stomach of the frog. Of the nerve fibres of the œsophagus, some terminate in the epithelium, probably between and not in the several epithelial cells, others surround the glands, and others supply the vessels. In the small arteries Goniaew distinguishes a wide-meshed adventitial network from a narrow-meshed muscular network. The latter is absent in the smaller veins. He also finds that a delicate terminal nervous plexus is extended between the capillaries, and that no free terminations can be discovered in this. All his researches were made by means of the gold method.

11. Herr Carl states that from his early youth he suffered from inflammation of his middle ear which followed an attack of diphtheria. The membrana tympani was entirely destroyed, and the mucous membrane of the wall of the tympanic cavity was in a state of cicatrisation. The power of hearing is, he states, much diminished,

the functions of the facial and trigeminal nerves are, however, completely preserved, the sensibility of the two anterior thirds of the left half of the tongue is perfect. Irritation of the chorda in the tympanum causes a sensation of pricking on the anterior part of the same side of the tongue. On the other hand, the gustatory sensibility of the anterior two-thirds of the left half of the tongue is completely destroyed. Taking into consideration the further fact that after mechanical irritation of the chorda in the tympanum, a well-marked secretion of saliva can be produced from the left *caruncula salivalis*. It appears to the author impossible, considering the small sectional area of the chorda tympani that the gustatory fibres should be alone destroyed whilst the sensory and secretory fibres are preserved. He therefore arrives at the conclusion that no lesion of the chorda has taken place in his case. On the other hand, he remarks that there are other nerves in the tympanic cavity, the destruction of which might immediately cause abolition of the sense of taste: These are the nerves composing the *plexus tympanicus*. A branch enters the tympanic cavity through the *canaliculus tympanicus* from the *ganglion petrosum* of the *glossopharyngeal* nerve, and here meets with branches of the fifth, which enter from the *otic ganglion* in the *nervus petrosus superficialis minor*. The chronic disease of the tympanic cavity destroyed this plexus, he thinks, but clinical observations have clearly shown, that a certain number of gustatory fibres run in the chorda; Carl in support of his opinion, admits a union of the *geniculate ganglion* of the facial nerve with the *plexus tympanicus*; that *geniculate ganglion* of the facial nerve, to which a branch of the *spheno-palatine ganglion* on the second division of the fifth is given off by the *nervus petrosus superficialis minor*. The path which he believes to be pursued by the gustatory fibres of the anterior portion of the tongue in passing to the brain is as follows: they first run in the *lingual branch* of the trigeminal, and whilst the greater portion of them reaches *via ganglia oticum*, the lesser *superficial petrosal nerve*, *tympanic plexus*, *ganglion petrosum* to the *glosso-pharyngeal nerve*, a variable portion enters the chorda, thus traverses the tympanic cavity, enters the facial nerve and passes with it in a centric direction to the *ganglion geniculatum*, from thence continuing its course it runs in the *ramus communicans* of the facial nerve to the *tympanic plexus*, and thus arrives for the second time into the tympanic cavity, together with that portion which proceeds from the *otic ganglion* to the *glossopharyngeal nerve*. In conclusion, Carl mentions that sensory impressions caused by irritation of the chorda in the tympanic cavity only affects the corresponding side of the tongue.

REPORT ON SURGERY.

By W. JOHNSON SMITH, F.R.C.S.,

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On Fractures of the Pelvis.—Dr. Riedinger, of Wurzburg, in a paper on fractures of the pelvis, states that these injuries, compared with fractures of other large bones, are of very rare occurrence. In a collection of tables of fractures taken from eight hospitals, which is published in Gurlt's Handbook, it will be found that out of 11,600 fractures of different bones, 98 only implicated the pelvis. Of 159 fractures treated at the Julius Hospital, Wurzburg, during twelve months, there was one only in which the pelvis was injured. The proportion of fractures of the pelvis to fractures of other bones is, according to Bardeleben, about 0·3 per cent.

Of the constituent bones of the pelvis, the one most frequently fractured is the pubis. Whether the horizontal or the descending ramus of this bone be more frequently broken is a point which has not been strictly determined. In five out of eight cases reported by Wernher, the descending ramus was the part implicated; on the other hand, in twelve cases collected by Streubel, there were ten in which the horizontal ramus, and two only in which the descending ramus had been fractured. Next to the pubis, in respect to frequency of rupture, follows the ilium, then the ischium, then the acetabulum, and finally the sacrum. In several cases there is fracture of two or more of the pelvic bones. Fractures of the pubis are, as a rule, oblique; those of the sacrum transverse. In the ilium the fracture may take any direction; in most instances the line is either vertical, dividing the bone into an anterior and a posterior portion of unequal size, or horizontal, with superior and inferior fragments also unequal. The crista ilii is sometimes detached *in toto* through injury, and may in addition be broken into an anterior and smaller fragment and a posterior and larger fragment. Either of the anterior spines of the ilium may also be broken off.

The majority of cases of fractured pelvis terminate fatally; some in consequence of concomitant injuries to the brain and spinal cord, others through internal hæmorrhage, and others again through wounding of pelvic viscera. Injury to the bladder or urethra is a frequent complication, especially when the horizontal ramus of the pubis is fractured. Although the prognosis is, as a rule, very unfavorable, a speedy and complete recovery may take place after severe injury and multiple fracture of the pelvis.

The author does not agree with those who hold that fracture of the pelvis is in every instance the result of direct external violence. He recognises the fact that the injury is often due to forcible pressure as in "buffer" and other railway accidents, to a severe blow on

the pelvis, or to a fall from an elevation, but at the same time believes that some forms of pelvic fracture may fairly be attributed to muscular action. The part played by muscular action and by forcible traction on the tendinous and ligamentous structures in the production of fracture has of late been much extended. It is now generally acknowledged by surgeons that detachment of the lower epiphysis of the radius, and fracture of a malleolus, of the neck of the femur, of the greater tuberosity of the humerus, of the patella, and of the olecranon, may be produced by traction. The anterior inferior spine of the ilium may, as has been proved by experiment on the dead body, be torn away by forcible traction on the ligamentum Bertini, and likewise the anterior superior spine may be detached through strong contractions of the sartorius muscle. In young subjects the crista ilii, which is but loosely connected to the rest of the bone, may be readily detached in consequence of a fall or blow. The author points out that in every case of such form of injury the whole of the epiphysis is detached, although the injuring force has often been applied but to a small portion. In order to explain this the author holds that another factor must be sought for, and that this is probably strong contraction of the muscles which are inserted into the outer surface of the crista, viz. the gluteus maximus and the gluteus medius, both strong muscles and capable of producing a separation of the crista. The patella, as is well known, is often broken through the contraction of the quadriceps extensor muscle, and the olecranon by the action of the triceps muscle. The occasional occurrence of a secondary fracture of the crista ilii into an anterior and smaller portion and a posterior and larger portion, may be explained by the fact that the gluteus maximus is inserted into the portion of bone which corresponds to one fragment, and the gluteus medius into the other and posterior portion. An outward inclination of the crista ilii favours its detachment through muscular contraction.

Fracture of the body of the ilium, dividing it into a superior and smaller fragment and an inferior and larger fragment, may, the author believes, be caused in some instances through the conjoined action of all the glutei muscles. This view may be more readily accepted if it be considered that the glutei muscles are very strong, and that the resistance of the bone is diminished by its outward inclination. From experiments on the dead body it has been found that fracture of the ilium produced through powerful pressure or violent blows takes usually a vertical or slightly oblique direction.—*Langenbeck's Archiv für Klinische Chirurgie*, Bd. xx, Heft. 2.

On Lister's Treatment of Wounds.—Dr. Lindpaintner, of Munich, through his own experience and observation of the results obtained in hospital practice from the application of Lister's dressings, has been led to form a very high opinion of their efficacy and value. With regard to the duration of the treatment in cases of wounds and of surgical operations, there cannot, the author states, be the slightest doubt that this may be much shortened by the practice of the antiseptic method. This has been shown lately in the Munich hospital

by almost every case of amputation, resection, and large surgical wound. The apparent contradiction to this statement, indicated by the fact that of late the average duration of treatment in the surgical wards at Munich has been increased, is accounted for by the fact, that the very severe cases of injury which formerly soon became fatal in consequence of acute pyæmia or gangrene helped to make a short average duration; whereas, at the present time, the majority of such cases recover under antiseptic treatment, but naturally demand a long period for the attainment of so good a result.

At first sight Lister's dressings seem to be much less economical than the kind of dressings formerly in use. On reflection, however, it will be found, the author holds, that the cost of surgical treatment in a large institution will be really reduced if the antiseptic method be practised.

Thus, formerly large doses of quinine were required for the reduction of high fever, narcotics were required to relieve pain, and, on account of exhaustion and poorness of blood through long-continued suppuration, iron, wine, and potent and costly nutritive agents were indispensable. Now severely-injured subjects are cured after but a short stay in hospital. Narcotics are seldom administered, since carbolic acid is itself an efficacious anæsthetic; quinine rarely needs to be administered, and the surgeon is no longer obliged to retain some of his patients for weeks, and even months, in order to remove the effects of extreme exhaustion and to restore them to a normal condition of health.—*Deutsche Zeitschrift für Chirurgie*, Bd. vii, Hft. 38, 39.

On Psoriasis of the Tongue.—The following general conclusions are given in a contribution by Dr. Nedopil, of Vienna, on psoriasis of the lingual and oral mucous membrane, and on the relation of this affection to carcinoma:

"1. Psoriasis of the mucous membrane of the tongue and oral cavity is caused through a chronic inflammatory process, which takes place in the proper mucous layer.

"2. This chronic irritative process results in a new formation of young indifferent cells.

"3. These indifferent cells, which lie near to the epithelial layer of the mucous membrane, are converted into true epithelium; hence results the formation of patches of abnormal, bulky, and rapidly regenerated epithelium.

"4. The epithelial layer of the mucous membrane, as in the normal condition of things, remains quite passive, and takes no part in the regeneration of these patches.

"5. The deeply-seated indifferent cells of new formation are partly converted into shrivelled, scar-like tissue, and partly persist as round cells, which may finally displace the proper tissue of the mucous layer.

"6. These indifferent cells when in the substance of the mucous layer, and before they have attained their ultimate position, may acquire an epithelial character, and thus carcinoma is developed.

"7. When carcinoma—that is to say, an epithelial new growth—is developed from indifferent cells, such a mode of nutrition as is met with at the surface only of the mucous layer under normal conditions of the tongue affects the cells contained in the deeper parts of this layer."—*Langenbeck's Archiv für Chirurgie*, Bd. xx, Hft. 2.

On Dupuytren's Finger Contraction.—Dr. Alfred C. Post, of New York, in an article on "Contraction of the Palmar Fascia and of the Sheaths of the Flexor Tendons," reports six cases of this affection, in which he operated with some success. Contraction of the palmar fascia is to be carefully diagnosed, the author points out, from persistent flexion of the fingers due to other causes, as to division of the extensor tendons by direct mechanical violence or sloughing, to cicatricial contraction after wounds or burns, to paralysis of the extensor muscles, or to spasmodic or organic contraction of the flexor muscles.

This disease, the true character of which was first demonstrated by Dupuytren, is usually, if not always, of traumatic origin, being the result of frequently repeated or protracted pressure of hard substances against the palmar integument. According to Dr. Post the integument of the palm, in consequence of the pressure to which it is subjected, becomes more or less inflamed and indurated, and adheres more firmly to the subjacent fascia which gradually contracts, so as to induce a permanent flexion of the fingers, at first to a slight degree, but progressively increasing until, in some cases, the ends of the fingers are almost in contact with the palm of the hand. Great resistance is offered to any attempt at extension, and severe pain is induced if much force be employed in this way. In the palm can be seen and felt hard fibrous cords formed by contracted bands of the palmar fascia together with the closely adherent integument. The ring finger is usually more affected than the others, and the little finger more than the index or middle finger. The affection may be confined to one hand, or involve both. The contraction of the palmar fascia is sometimes accompanied by a contraction of the sheath of one or more of the flexor tendons, a very obstinate and unyielding flexion of the affected finger being thus produced. Dr. Post, whose attention has been directed to palmar contraction for several years past, holds that Dupuytren, in his views as to its pathology, erred in excluding contraction of the sheaths of the tendons from any participation in the production of the deformity. Whilst admitting that there are many cases in which contraction of the palmar fascia is the exclusive cause of the persistent flexion of the fingers, he hardly thinks it possible that his observation could have deceived him as to the fact that, in some cases, the tendinous sheath is also at fault.

The surgical treatment of this affection consists in division of the contracted bands, extension of the affected fingers, and their retention in this position by proper splints. By a subsequent vigorous and persistent application of passive motion the deformity, the author states, may be effectually overcome, and a very useful degree of motion may be restored. He does not believe, however, that perfect

freedom of motion can readily be regained. The operation of dividing the contracted bands being a painful one, an anæsthetic is usually required. The after-treatment is also painful, and needs to be kept up for several months. The patient, therefore, has no trifling infliction to undergo before his affliction can be relieved.

Dr. Post, in dividing the contracted bands of the palmar fascia, makes his incisions at a number of points, and wherever they are capable of relieving tension. The adhesion of the palmar fascia to the skin is so close, it is stated, that a strictly subcutaneous section cannot be made. Passive motion is regarded as an important means of restoring mobility to the fingers.—*Archives of Clinical Surgery*, New York, No. 2, 1876.

On Trephining in fracture of the Cranium.—M. C. Sedillot has recently put forth a further contribution in support of his view that in fracture of the internal table of the cranial vault with displacement of fragments, trephining is the only means of preventing complications that are almost invariably fatal. Of 106 cases of fracture of the cranium collected by the author, there are seventy-seven in which trephining was practised and twenty-nine in which no operation was performed. In nine cases the operation was *preventive*, that is to say, it was performed before the manifestation of any bad symptoms. In sixty-eight cases it was *curative* and had as its aim the relief of serious complications, such as paralysis, loss of consciousness, convulsions and coma. In twenty-one of the latter set of cases the operation was performed before the sixth day from the receipt of injury, and in the remaining forty-seven cases after this period. In all the 106 cases the internal table was fractured. In twenty-one cases the external table was not involved, and in most of these the symptoms were so slight that the gravity of the injury was overlooked.

Out of the twenty-nine cases of comminuted fracture of the inner table in which trephining was not performed, there was one recovery only and twenty-eight deaths. Of the seventy-seven patients who were trephined thirty recovered and forty-seven died. In nine of these cases the operation was performed for prevention of serious symptoms; in six with success, in three with a fatal result. In sixty-eight cases the operation was *curative* and resulted in twenty-four of them in recovery, and in forty-four in death. Of the twenty-one cases in which the operation was performed before the sixth day from the date of injury, eight ended in recovery and thirteen in death. Of forty-seven patients trephined on or after the sixth day, fifteen recovered and thirty-two died. The mortality, M. Sedillot points, bears a direct proportion to the delay in the application of the trephine. Two thirds of the number of patients were cured by "preventive" trephining, more than one third by *early* trephining (before the sixth day), and less than one third by *late* trephining (on or after the sixth day). Of the twenty-nine cases in which the operation was not performed, one only ended in recovery. These figures show, according to the author, that comminuted fracture of the inner table of the skull is an injury

almost invariably fatal if no attempts be made to elevate and to remove the fragments.—*Gazette Médicale de Paris*, No. 39, 1876.

On the Immediate Capillary Hæmorrhage produced through Esmarch's Method.—Dr. Nicaise has suggested a plan of preventing the free bleeding so often observed after an operation performed on a part rendered bloodless by the application of Esmarch's elastic bandage and ligature. This method, as is well known, determines in the region to which it has been applied a vaso-motor paralysis which affects the whole thickness of a limb, and manifests itself immediately after the removal of the compressing ligature by two phenomena: viz. congestion of the integument and, in case of an operation, capillary hæmorrhage from all parts of the surface of the wound. This congestion is produced rapidly in the part of the limb below the seat of the ligature; the intensity and duration of the congestion bearing a relation to the force and duration of the previous compression. It is manifested on the surface of the wound by oozing of blood which is sometimes very free. This bleeding comes on immediately after the removal of the ligature and persists for as long a time as the congestion of the skin; the flow of blood and the tegumentary redness gradually disappearing together, as both phenomena are due to the same cause. For the prevention of this troublesome bleeding after operation some French surgeons have applied a firm compressive dressing before the removal of the elastic ligature, and others merely a solution of perchloride of iron. Dr. Nicaise does not approve of either of these plans, but recommends the following instead: All visible arteries having been tied and the elastic ligature then removed from the limb, a large sponge dipped in a solution of carbolic acid (1 to 50) is to be applied over the surface of the wound and firmly retained there until the redness and congestion of the skin has passed off. It will generally be necessary, Dr. Nicaise states, to hold this sponge on the raw surface for eight or ten minutes. Then, on the removal of the sponge, the wound will be found exsanguine and dry, save, as in a major amputation, at the site of some few smaller arteries overlooked before the removal of the elastic ligature.

Through supplementing Esmarch's method by compression with a sponge on the surface of the wound, the surgeon may perform even major operations with but little bleeding. Dr. Nicaise states that in a recent amputation of the thigh his patient lost scarcely 150 grammes of blood. An amputation performed under these conditions would then, as Dr. Nicaise points out, comprehend these three stages: 1. Application of the elastic bandage; application of the annular elastic ligature; removal of the bandage; 2. The operation; deligation of vessels; 3. Compression of the surface of the wound by the sponge, followed immediately by removal of the elastic ligature.—*Gazette Médicale de Paris*, 34, 1876.

On Vaginal Lithotomy.—Dr. J. Collins Warren, of Massachusetts, writes in favour of vaginal lithotomy, an operation which has been performed by a number of American surgeons, and, indeed, seems to have been done more frequently in the United States than in any

other country. This operation, though no novelty, has of late been placed on an altogether different footing by the employment of sutures to close the incision connecting the bladder with the vagina. The operation has not been confined to the adult, and a number of cases have been reported where it had been performed upon young children. The author at his first effort was struck by the fact that this is not a difficult operation to perform. The patient being placed in the position for lithotomy, when the labia are separated the anterior wall of the vagina may be seen hanging like a curtain near the vulva. The operation, it is stated, appears to be attended with little or no danger, hardly a single death having been reported. In cases where the attending inflammation is severe, vaginal lithotomy not only removes the cause, but exerts a curative influence upon the inflammation itself, provided the surgeon simply avoids using the suture. The only real danger is from a failure of union of the wound, and this, at the present time, has been reduced to a minimum. Incontinence resulting from this cause is far more easily remedied than that which follows from over-distension and paralysis of the urethra.

The author seems to be opposed to the operation of extracting the stone through the dilated female urethra, and he refers to the opinions and published cases of many surgeons, both American and British, indicating the risk of obstinate incontinence. According to the late Professor Simon, of Heidelberg, the limit beyond which the female urethra cannot be dilated without running the risk of incontinence is 2 centimètres in width and 6.3 centimètres in circumference. Dilatation to this size would permit the index finger of most surgeons to be passed with ease into the bladder. Nothing but a very small instrument could be passed at the same time without stretching beyond this limit, and a stone, as Dr. Warren states, must obviously be a small one to be removed by an instrument delicate enough to pass so narrow a space. It is pointed out that in some instances death has occurred within a short time after simple dilatation of the female urethra in the subjects of renal disease.

Lithotrity, the author acknowledges, is undoubtedly as simple and harmless an operation as any, in the hands of a skilful surgeon, provided its use be limited to a certain range of cases. The operation is not without risks. A case is alluded to of a woman who died from exhaustion after crushing of a very large stone, and who might have been saved, it is thought, had vaginal lithotomy been performed. The alternative in the male is a capital operation; in the female, on the other hand, one which has been placed within the domain of minor surgery. We are not, therefore, Dr. Warren argues, authorised in females, as we are in males, to run any great risk. It is thought that lithotrity should be confined to cases where the stone is neither large nor hard. Dilatation of the urethra, combined with lithotrity, would prevent the removal of a much larger stone at one operation than by lithotrity alone, but the dilatation must not exceed the limit given by Simon. If the stone be

hard, but small enough, it may be extracted whole through the dilated urethra. If, on the other hand, the stone is large, and there is much cystitis, or the patient is feeble, or there is reason to expect trouble in the kidneys, vaginal lithotomy, Dr. Warren holds, should be the operation.

In conclusion, it is stated that vaginal lithotomy, involving at the most a danger completely within control, may be employed in a much wider range of cases than it has been hitherto; while dilatation of the urethra, entailing, it may be, an infliction which cannot be remedied, should be practised with great caution until the class of cases to which it is suited are more fully understood and the limit to which it can be carried determined with greater certainty. *Boston Medical and Surgical Journal*, July 20th, 1876.

On Thoracentesis.—Dr. S. C. Chew, of Baltimore, though valuing the operation of paracentesis as undoubtedly the only hope in some cases of restoring the function of a lung, and thus of ultimately saving life, is fully persuaded that the advocacy of this measure has been carried too far, and that there is need for a recall from the shorter course of surgical procedure to the older and more reliable methods of medicine. It is held that there are limits beyond which its use cannot be carried except at the risk of doing more harm than good. Three classes of cases are given in which the question of thoracentesis—and by this term Dr. Chew means pneumatic aspiration—must be entertained. The first class consists of those cases in which a large pleuritic effusion has suddenly occurred, and has overwhelmed the function of one lung before there has been time for the supplementary action of the other to be brought fully into play; or, again, where hydrothorax, as distinguished from pleuritic effusion, has occupied both sides of the chest in such amount that respiration is dangerously embarrassed. In this class of cases thoracentesis must be immediately performed. The second class is composed of cases in which, from the physical signs, one might be tempted to perform thoracentesis, but in which the clinical state made out by careful examination of the chest does not demand immediate surgical interference. The rapidity with which effusion collects in a pleural cavity is a more important factor in the production of dyspnœa than the amount of the fluid effused. In those cases, if the condition has not continued long, it is best, Dr. Chew believes, to use medical means. The author does not hold with those who contend that even in these cases, where there is no urgent need for interference, better, because quicker, results may be obtained through surgical operation than through medical means. It is pointed out that if the vessels of the pleural surfaces, on the removal of the fluid, remain still in the same condition in which they were when it was effused, the collection of fluid will be again and again renewed, and need arise for repeated surgical operation. Again, if adhesion of the compressed lung has taken place, the sudden entrance of an increased amount of blood into the pulmonary vessels, and of air into the vesicles, may produce a strain upon the lung-tissue now unsupported, and so give rise to that sense of pain-

ful constriction, together with a tendency to syncope, which patients occasionally suffer during and after the operation. Cases of sudden death from thoracentesis have of late been reported in French medical journals, and a statement is current, though not hitherto proved true by accurate statistics, that there has been an increase in the mortality from pleurisy since thoracentesis has been more generally practised. The third class of cases consists of those in which, as in the first class, thoracentesis is plainly required as the only means likely to afford relief. The following are the several conditions grouped together by Dr. Chew in this third class: 1. Where the purulent character of the effusion is indicated with certainty or with high probability. 2. Where an effusion reaching half-way up one side of the chest is undiminished by medical treatment persistently applied for three or four weeks; and, *a fortiori*, if it have increased in spite of such treatment. 3. Where, with a large effusion, previously not alarming in character, sudden and urgent dyspnoea has occurred. Dr. Chew insists on the importance of being able to decide whether the fluid be purulent or serous; for, the purulent nature of the fluid being once determined, it is useless to continue medication with a view to its removal. In doubtful cases, mechanical evacuation will constitute a diagnostic as well as a therapeutic means. As it is sometimes desirable to determine the question before resorting to any kind of operation, it should be remembered that assistance is promised by Baccelli's test, founded on the physical law, that the vibrations of sound in liquids are transmitted *inversely* to their density,—the sound passing more readily through a serous than through a purulent fluid. Dr. Chew asserts that there is certainly a marked difference in the vibrations transmitted through the two fluids respectively, and that feebleness or absence of vocal or respiratory sounds is a highly probable indication that the fluid is pus.—*New York Medical Journal*, No. 3, 1876.

On Trephining in the different Forms of Osteo-myelitis.—M. Ollier, in a recent communication to the Academy of Sciences of Paris, holds that trephining is applicable to all forms of osteo-myelitis which have, as a predominant character, intense and persistent pain. The operation is also applicable, under certain conditions, to acute osteo-myelitis with severe general symptoms. Intense and persistent pain is not, M. Ollier states, peculiar to a single variety of osteo-myelitis. The most diverse lesions of the bone-marrow may have a neuralgic character, the pain being the result of compression of the inflamed and swollen medullary tissue by the surrounding bone. In different cases of painful osteo-myelitis the bone-marrow, when exposed by trephining, presents the most diverse appearances. In some instances there will be found a fungous mass bathed in serous fluid and enclosed in an osseous capsule. In other cases the medulla will be firm and gelatinous, and be traversed by trabeculae of newly-formed bone; or, again, the surgeon may strike on a regular cavity with smooth walls, and filled by pus. When the osteo-myelitis is of slight extent, and the disease is circumscribed by an osseous wall, immediate and decided relief may usually be obtained

by trephining. In the plastic form of osteo-myelitis, accompanied by more or less eburnation of the peripheral layers, relief is less speedy and less certain, except a considerable portion of bone be removed. When the foci of disease are disseminated, the causes of the compression of the bone-marrow will, probably in part, be left without the track of the trephine. In this diffuse form, however, the operation may arrest for a time the intolerable pain, and enable the patient to live in ease for several months, and even for years. Painful osteo-myelitis is generally met with near an end of the shaft of a long limb-bone (tibia, radius, femur).

The name of *epiphysial osteitis* cannot be properly applied to painful osteo-myelitis near one end of a long bone. An epiphysis is rarely the primary seat of the affection, and, when it is, the neighbouring articulation is also involved. The seat of election of painful osteo-myelitis is the spongy part of the diaphysis, near the cartilage of conjunction.

Notwithstanding its brilliant results, trephining, M. Ollier states, should not be performed until after the non-operative resources of treatment have been exhausted. In some cases the pain may be relieved by simple separation of periosteum, and without perforation of bone. Trephining should be reserved for those cases in which the inflammatory character of the lesion cannot be doubted. The operation may, when other plans of treatment have failed, be applied in any form of inflammation, if the severity of the symptoms demand intervention. In the acute form the grave and often fatal symptoms of suppurative osteo-myelitis may be prevented, and in the subacute or chronic forms, the predominant phenomenon of which is intense and obstinate pain, relief may be afforded by relaxing the pressure of the peripheral osseous tissue on the swollen marrow. The trephine, in its course to the medullary spaces, will traverse in some cases thickened and more or less eburnated bone, and in other cases bone that has become soft and atrophied. The condition of thickening is met with generally in old cases in which prolonged plastic transformations have taken place around the primary seat of disease; the condition of softening in cases of recent subacute inflammation. In the former class of cases spontaneous cure is almost quite impossible by reason of the resistance of the osseous walls. In the latter class pus will gradually find a way to the surface of the bone through gradual "medullisation" of the wall of the shaft. This process of natural cure, however, may be carried over a long period, and may keep up for many months' suffering that might at any time have been instantaneously relieved by surgical intervention.

In eight out of nineteen cases of painful osteo-myelitis treated by trephining M. Ollier found pus. In ten cases were found the different morbid conditions of the marrow that have been mentioned. In three out of these ten cases there was a distinct and regular cavity. In the remaining seven cases the disease was not well circumscribed. In the last case—one of acute osteo-myelitis of the femur—M. Ollier gave exit to a large collection of fluid blood. The result of the operation, in most of these cases, was very simple. The type and

character of the pain were at once changed, and the nocturnal attacks of deep-seated and lancinating anguish were replaced by a feeling of uneasiness in the wound, which gradually ceased. This change took place most rapidly in those cases in which a circumscribed cavity had been found in the bone.

Of the nineteen cases of trephining alluded to by M. Ollier two were fatal through pyæmia. In one of these cases osteo-myelitis persisted after the operation; in the other case the patient died on the sixty-third day from the date of operation after convalescence, as it was thought, had been well established, and the wound had almost completely closed.—*Gazette Médicale de Paris*, No. 36, 1876.

On "*Cellulo-vascular*," *Polypi of the Urinary Meatus*.—The following is an abstract of some clinical remarks made by M. Gosselin of the *Hôpital de la Charité*, on two cases of urethral polypoid growths in the female.

In each case there was at the external urinary meatus a small red granular prominence resembling an overgrown granulation, or one of the small vascular tumours that are sometimes developed on the neck or within the cavity of the uterus,—tumours that have been called vascular polypi, but incorrectly, since they differ from ordinary polypi of the womb in being less voluminous, and in not containing uterine fibres and muscular tissue. To these growths of the meatus, M. Gosselin applies the name of "polypi" in conformity with general custom, but he takes care to point out they cannot be strictly regarded as such, since they are attached to the mucous membrane of the urethra by a broad base, and do not present a well-marked narrow pedicle. A similar exception in nomenclature is generally made with regard to the so-called naso-pharyngeal fibrous polypi, which have a too extensive attachment to allow of their being classed as true polypoid growths. Nélaton, indeed, proposed for them the denomination of fibromata of the base of the cranium. The tumours removed in M. Gosselin's two cases were found to be made up of embryonic cells without any admixture of fusiform fibres. The growths were more vascular, but in every other respect were constituted like ordinary mucous polypi. These tumours of the meatus do not involve the whole contour of the urethra, or spread to the vagina as cancerous growths do. But it very often happens that they rapidly recur after excision. This recurrence is probably due to the fact that the base of the tumour is large and extended for some distance along the urethra, and that a portion of this base may be left after operation. It is in respect only to this recurrence that these polypoid growths of the female urinary meatus are analogous to cancerous formations. The removed growth takes place only in the urethral mucous membranes; the glands do not become engorged, and there is never any cachexia. This tumour of the meatus is a vascular mucous polypus, and is essentially benign in its nature. In one of the cases referred to by M. Gosselin, the subject of which was fifty-five years of age, the tumour of the meatus was quite indolent. The patient suffered neither in walking nor during micturition, and there was no absolute indication for the

removal of the small vascular growth. In the second case the tumour, though resembling that in the first-named case in size, nature and composition, gave rise to acute suffering. The woman had intense pain when she walked, when the tumour was touched, and both during and for some time after micturition. Of this difference between the tumours in the two cases, M. Gosselin can give no explanation beyond the assumption that it was due to idiosyncrasy or individual variety. An analogous difference in character is presented in cases of erythema of the vulva. In some patients the eruption is absolutely indolent, in others it is associated with intolerable pruritus, and very intense pains about the vulva.—*Gazette des Hôpitaux*, No. 112, 1876.

On Intra-articular Injuries of the Knee—Prof. Dittel, of Vienna, has obtained from experiments on the dead body and from clinical observations, the following points of information with respect to intra-articular injuries of the knee:

“1. The crucial ligaments are never torn through; but by violent movements of the bones carried beyond the functional limits they may be separated at their attachments. It is almost always the femoral end that is torn away—that of the posterior ligament by over-extension, that of the anterior ligament by over-flexion.

“2. The lateral ligaments may, by hyperabduction or hyperadduction, be torn through or detached either at the femoral or at the tibial end. There will be a separation in the former case of a scale of bone, in the latter of the corresponding semilunar cartilage. The lateral ligaments remain intact under those conditions which produce lesions of the crucial ligaments.

“3. Through violent movement the anterior crucial ligament may, as has been shown in a clinical instance, be torn away at its tibial end, carrying with it the spine of the tibia together with a disc of bone.

“4. Through violent movement in an opposite direction, the posterior crucial ligament together with a portion of bone, may be torn away from the tibia.

“5. Considerable displacement of the tibial epiphysis forwards in a flexed condition of the leg corresponds to the lesion of the anterior crucial ligament; displacement posteriorly, and into the popliteal space corresponds to the lesion of the posterior crucial ligament.

“6. An abnormal range of the movements of abduction and adduction corresponds to lesion of the lateral ligaments. Whether these be torn through or detached at their insertion may be determined, if there is no swelling of the soft parts, by digital examination and from the presence or absence of a depression.

“7. That separation of a crucial ligament at its long attachment should take place at its femoral end, and hardly ever at its tibial end, is probably due to the fact that the femoral insertion is small, quite circumscribed, and surrounded by articular cartilage, whilst the tibial insertion sends out ramifying fascicles of fibrous tissue by which it is connected to the periosteum.”

In discussing the diagnostic signs of fracture of the spine of the tibia, Prof. Dittel states that crepitation cannot be made out with any certainty, and that the free and abnormal mobility of the head of the tibia that one would expect after separation of the anterior crucial ligament, is not always evident in the living subject. The osseous fragment to which the lower extremity of this ligament always adheres may not have been completely detached from the head of the tibia, and in any case the surgeon would naturally be unwilling to convince himself as to the abnormal mobility, from fear lest by too energetic action he might cause pain to the patient, and convert what might probably be only a partial fracture into a complete separation. The most important symptom is a sudden and abundant accumulation of blood in the capsule, which after the use of the aspirator will be rapidly renewed. Such bleeding may, the author grants, be the result of bruising and laceration of the vessels which supply the capsule and other ligaments, and probably also of a tearing of cartilage, but as bleeding from such sources is usually derived from the smallest vessels, one would not expect it to be renewed after aspiration. On the other hand, bleeding due to rupture of vessels on bone would persist, and form repeated accumulations, since such vessels cannot readily contract and their orifices are not so likely to be closed at an early period through thrombosis. This renewal of the bleeding after the use of the aspirator in cases of injury to the knee, is regarded by the author as an important diagnostic indication of fracture, although it can afford no help in the determination of the precise seat of the injury.—*Stricker's Jahrbücher*, hft. 3, 1876.

On Glioma of the Retina.—MM. Warlomont and Duwez state that according to recent researches there is but one neoplasma, viz. glioma, capable of being developed in the retina. Anatomically this neoplasm consists of a number of small nucleated corpuscles, resembling somewhat those of the granular layer of the retina, and imbedded in a very scanty amorphous stroma. Glioma in the retina, as in the brain, is developed not from the nervous elements of the structure in which it arises, but from the neuroglia and at the expense of connective-tissue elements. Retinal glioma constitutes at first a purely local affection, but finally causes death, after spreading not only by local propagation to the choroid, the optic nerve, the orbit, and the cranial cavity, but also by metastasis to the lymphatic glands, liver, kidney and bones. The cranial contents become sooner or later affected through propagation of the disease along the optic nerve, or through destruction of the walls of the orbit, or through the development of metastatic foci in the diploe and under the periosteum of the cranial bones, and in the substance of the brain itself. The primary growth after filling the interior of the eye-ball protrudes in front through a perforation of the cornea, sometimes, though much less frequently, through the sclerotic, and forms a large fungous tumour.

It has been calculated by Dr. Landsberg, of Berlin, that the

proportion of cases of glioma to cases of all other affections of the eye is as 1 to 1937.

The authors, in describing the clinical history of glioma, note three periods in the development of the disease. In the first period the external form of the eye is unaltered and the patient is able to see through the cornea and pupil. In the second period the form of the eye has undergone change; the organ is much enlarged and seems ready to burst. In the third stage, that of fungous protrusion, the eye is completely destroyed. The first period is not generally accompanied by pain or external inflammation. In some few cases, however, the first symptom that is observed is inflammation of the eye, with epiphora and cephalalgia. Occasionally strabismus is the first symptom that attracts attention. Vision is completely lost from the first. It sometimes happens that this affection is treated for months as an ophthalmia without any suspicion being entertained as to the true nature of the case. In the early stage of glioma of the retina the iris loses its natural colour; behind the slightly irregular immovable and dilated pupil a white or yellowish-red reflection may be observed; and the bottom of the eye when illuminated by a light not very intense, resembles that of a cat or sheep. On ophthalmoscopic examination there may be seen at the fundus a circumscribed prominence of the retina, of a light yellow, or bright white colour, and traversed on its surface by numerous vessels which take a tortuous course. This prominence is not usually smooth, but in the majority of instances presents an irregularly lobed surface.

In the second stage, during which the growth advances from the fundus to the cornea, the eye-ball becomes fixed in the orbit, increases in size, loses its rounded and regular form, and, in consequence of the yielding here and there of the sclerotic, presents several prominences on its surface. These changes are associated with some pain in the eye-ball, epiphora, conjunctivitis, and chemosis. The patient is troubled also by violent attacks of pain in the forehead and back of the neck; these are more intense in the night than in the day, and by involving the head may determine delirium.

The third stage is that of fungous protrusion, this usually terminating in an attack of coma, or of convulsions, by which the patient, much exhausted through hæmorrhages and hectic fever, is carried off. The duration of the disease is variable; in some cases it lasts but for a few weeks, in other cases for several years. Its progress is less rapid in adults than in infants. The second and third periods are usually more rapid than the first.

Glioma of the retina is observed more frequently in children than in adults. The subjects of twenty out of twenty-four cases collected by Mr. Warlomont were under the age of twelve years. In the majority of cases the subject is between the ages of two and four years. The disease has been observed within a few months after birth, and, in one case, in an infant of nine weeks. The mother of this child had noticed the disease six weeks previously, so probably in this case it was congenital. The infants who succumb

to glioma of the retina have generally a well-marked scrofulous temperament, or belong to scrofulous families. In many of the reported cases there is a history of a blow on the eye having preceded and apparently excited the disease. It may be assumed, however, that the early loss of vision due to the latent disease renders the patient more liable to such an injury. An examination of the eye for the purpose of making out the supposed results of this injury will reveal signs of the disease which had previously existed without attracting attention.

The correct diagnosis of glioma at an early stage is of importance. When the disease is circumscribed and has not yet involved much of the optic nerve, enucleation of the globe may be practised with good chances of permanent success. The determination of the nature of the disease is seldom attended with difficulty. The only affections with which it can be confounded are these: detachment of the retina, the results of parenchymatous choroiditis, sub-retinal cysticercus, the results of suppurative hyalitis. Detachment of the retina is hardly ever observed in infancy. The prominence is smooth, and of a grey or opal colour, and the vessels which traverse its surface present the normal conditions of the vessels observed at the fundus of the eye-ball: they are neither more nor less numerous than those of the healthy retina, as they follow almost the same track. Glioma in its earliest stage is never associated with cerebral symptoms, ocular tension, and synochia, which phenomena are, as a rule, associated with parenchymatous choroiditis. According to Knapp there is on solar illumination of the fundus, a greyish-white reflection with the latter affection, and a bright yellow reflection with glioma. In cases of sub-retinal cysticercus and of old suppurative hyalitis due to the presence of a foreign body, the bright yellow or coppery reflection observed on heliostatic illumination of the gliomatous eye is always absent. Besides, cysticercus is never met with in the infant, and in cases of suppurative hyalitis there is always a history of injury.

The only treatment of glioma of the retina is extirpation of the eye-ball. This operation, when practised in good time, offers such chances of success that the surgeon is hardly justified in refusing to undertake it. When it is done, whether early or late, the optic nerve should be divided as far as possible from the globe. The results hitherto obtained from this practice have not been discouraging.—*Annales d'Oculistique*, Juillet, Août, 1876.

REPORT ON PHYSIOLOGICAL AND PATHOLOGICAL
CHEMISTRY.

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THE excellent manual of Gorup-Besanez has now appeared in a French translation under the title 'Traité d'Analyse Zoochimique.' For ordinary physiological laboratory work it leaves nothing to be desired, but we miss some of the more novel and effective processes of research which have lately led to fresh discoveries in animal chemistry. The work of H. Beaunis, entitled 'Nouveaux Eléments de Physiologie Humaine,' has been already reviewed in our pages. It contains a vast amount of information on the subject of physiological chemistry.

The present reporter has prepared a small work on 'Food' for the Educational Committee of the Privy Council (Chapman and Hall).

Chemistry of the Brain.—Very important researches on the chemical constitution of the brain have been published by Dr. Thudichum ('Reports of the Medical Officer of the Privy Council,' New Series, No. III, pp. 113 to 247). His elaborate paper is not only of extreme interest and value from the original results which he has obtained, and from the careful descriptions given of his methods of preparations, but also from his critical and historical account of previous researches. We must refer our readers to the original memoir (a blue-book costing no more than 1s. 4d.) for full details of Dr. Thudichum's labours, but we cannot refrain from making an attempt to condense still further the compact summary of his results which the author has himself furnished.

The brain contains its large percentage of water in great part in the form of hydration or colloidal water. It is not there as in a sponge or as a serum, but far more intimately associated with the complex organic compounds of the brain. These organic compounds are not necessarily peculiar to the brain or nerves, though some of them may be. They form three groups, the members of the first of which contain carbon, hydrogen, oxygen, nitrogen, and phosphorus, and may be termed phosphorized bodies; the members of the second group contain four of the above elements, but no phosphorus, and are named nitrogenized bodies; the members of the third group contain carbon, hydrogen, and oxygen only. The most important members of the phosphorized group are the various forms and derivatives of the substances designated by Dr. Thudichum *kephalin* and *myelin*, with the previously described substance called *lecithin*.

In all of these bodies the phosphorus exists as phosphoric, or rather as glycerophosphoric, acid, and not as the unoxidized ele-

ment. Kephalin is easily oxidized, myelin is more permanent, but lecythin is peculiarly prone to decomposition. All the forms of these three bodies have powerful attractions for water and for certain mineral salts.

The nitrogenized bodies of the brain are far more stable compounds than the phosphorized. Like the latter, they are colloids, and do not pass the septum of a dialyser, or even a filter paper. They are not even seemingly soluble in water, though they gelatinize by absorbing it. They are soluble in hot alcohol or hot benzole, from which they separate in slightly crystalline forms. They may be made to yield a few substitution products, but not any isolable or definite compounds with salts, acids, or oxides. There are three subgroups of these nitrogenized principles—*cerebrin*, *phrenosin*, and *kerasin*.

The oxygenized compounds of the brain include a considerable quantity of cholesterin. But Dr. Thudichum has devoted most of his attention to the two previous groups, which are, indeed, far more difficult to study than such bodies as cholesterin and inosite.

We ought to add that the author is convinced of the invariable presence of copper and manganese in brain substance, and that he has worked chiefly upon ox brains, though not exclusively.

Gobley, to whom much credit is due for his very considerable discoveries in the chemistry of the brain, has recently ('*Journ. Pharm. Chem.*' (4), xx, 161, 1874) given an approximate quantitative analysis of brain. His figures seem to be not wholly compatible with the results of Thudichum. He, however, considers that he has proved the presence in brain of ordinary soluble albumen.

Chemistry of Food, and Digestion.—The relations of chlorides and of lactic acid to the process of digestion have been studied by several physiologists in different ways. R. Maly, experimenting with dogs ('*Ann. Chem.*' clxxiii, 227), concludes that lactic acid is not a constituent of pure gastric juice, nor is it concerned in the separation of hydrochloric acid from the chlorides of the food. His elaborate paper leaves the problem undecided, but his numerous experiments tend to show that the living mucous membrane of the stomach does not necessarily induce the lactic fermentation in solution of sugar.

The numerous analyses of ox and horse flesh made by J. Leyder and J. Pyro ('*Archives Pharm.*' (3), vii, 150), are of interest when taken in connection with such previous inquiries as those of Lawes and Gilbert, Payen and Mène. One of the chief defects of most analyses of the flesh of animals used as food is the imperfect way in which the nitrogenous constituents are described. In all cases the gelatinous substances or osseids should be separately determined and discriminated from the true albuminoids, while the amount of other nitrogen compounds, such as creatine, should be recorded.

The mutual relations of sodium to potassium salts in food and in the urine have been noticed in previous reports. Further researches by A. Dehn confirm and extend those of previous observers ('*Pflüger's Archiv*,' xiii, 353). His conclusions are briefly as follows:—Under ordinary conditions of diet all the potas-

sium of the urine exists as chloride, the ratio of this salt to the sodium chloride being as 1:1.35. When potassium salts (as the phosphate) are introduced into the system in considerable excess the blood is robbed of its chlorine that potassium chloride may be formed. But Dehn finds that sodium chloride is withdrawn from the system, even by the administration of potassium chloride. This salt, as well as articles of food rich in potassium compounds, causes some increase of tissue change, and consequently of the elimination of urea. Both beer and coffee often contain enough potassium salt to produce some of the characteristic effects traceable to this metal.

It cannot yet be said that a definite position as nutrient materials can be assigned to the osseids, the substances of which we have familiar examples in gelatin, in cartilage, and in the ossein of bones. The latter substance seems a somewhat better form of nitrogenous substance than the gelatin, to which it gives rise. The members of this group, although incapable of replacing the true albuminoids, are yet not without use as economising the latter (C. Voit, 'Zeitschrift f. Biologie,' x, 202 and 84, for the Memoir, by J. Etzinger).

E. Weiske and E. Wildt have studied, by means of experiments on pigs, the origin of the fat of the animal body. Their results, though tending to confirm the hypothesis of C. Voit, that the nitrogenous principles of food, together with the existing fat therein, are the sole sources of the fat of the body, and that the carbohydrates do not furnish this constituent (*ibid.*, x, 1) are not final.

Peptones, previously prepared from albumen, and used as the sole nitrogenous constituent of food, would appear to be capable of nourishing the body exactly in the same manner as when the original albumen is introduced as such into the organism. This result of P. Plósz and A. Gyergyai ('Pflüger's Archiv,' ix, 323, and x, 536) does not in reality contradict those experiments of Brücke, Fick, and others, which have been assumed to countenance the absorption of unaltered albumen in the body, yet it certainly renders this inference less probable.

Pure peptone gives no precipitate with acetic acid and potassium ferrocyanide, according to A. Kossel (*ibid.*, xiii, 309). R. Maly regards peptones as perfectly homogeneous and identical in composition with the albuminoids from which they have been prepared. Nor does he think that their molecules are less complex than those of their originating albuminoids, which they can, indeed, regenerate (*ibid.*, ix, 585). B. Kistiakowsky is not of this opinion. He regards the peptones resulting from the pancreatic digestion of ox-blood fibrin and of vegetable casein as containing not only more oxygen, but less carbon, than the original albuminoids. The fluorescence of peptones is spoken of by this experimenter as a marked characteristic (*ibid.*, ix, 438).

The frequent occurrence of large quantities of marsh gas in the intestines of the herbivora has been long known. A vegetable diet, in which the more alterable forms of cellulose as well as mucilage and gum abound, produces a similar result with the human subject.

This marsh-gas fermentation has been studied, especially in relation to its occurrence in the materials of the solid excreta of man, by I. Popoff (*ibid.*, x, 113).

Chemistry of the Liver and of Bile.—That the liver is not the main seat of the formation of urea is the conclusion deduced by T. Munk (*ibid.*, xi, 100), from the analysis of the blood and livers of dogs. The percentage of urea in the liver was always *less* than that in the blood.

The colouring matters of the bile still continue to attract much study. Dr. Thudichum's further researches on bilirubin are a real addition to our knowledge of this intricate subject ('*J. Chem. Soc.*,' ii, 1876, p. 389).

A. Heynsius has discussed some of the views of himself and others on bilirubin, cholecyanin, &c. ('*Pflüger's Archiv*,' x, 246). L. Liebermann has investigated the optical and chemical characters and relationships of two important derivatives of bilirubin, namely, choletelin and hydrobilirubin. He proves that these compounds are perfectly distinct from each other and from bilirubin, and that, given any one of these colouring matters, the other two may be prepared from it by processes of reduction or oxidation (*ibid.*, xi, 181).

Chemistry of the Blood.—C. Paquelin and L. Jolly claim to have prepared the colouring matter of blood in a state of purity, and yet free from iron ('*Comptes Rendus*,' lxxix, 918); but their treatment of the blood corpuscles and of the substances extracted therefrom was of so violent a kind as to alter most profoundly the hæmoglobin originally present therein. Thus, the material they finally obtained and analysed was a mere decomposition product of minor interest; and Thudichum and Kingzett ('*J. Chem. Soc.*,' vol. ii, 1876, p. 260) have quite recently shown that the above authors are not only wrong in regarding the red matter they described as the true colouring matter of blood, but have been misled as to the iron in it. Of this metal it really contains 9 per cent.

A process for the isolation of the true colouring matter of the blood (to which the name hæmoglobin is now pretty generally applied) has been devised by Béchamp ('*Ann. Chim. Phys.*' (5), iii, 340). Until his preparations have been submitted to analysis and spectroscopic study it will be better to suspend our judgment as to the value of his methods.

The non-removal of carbon dioxide from air which is being respired lessens physiological oxidation within the body and the elimination of the above-named gas; but this result is in a measure counteracted by the increased number of inspirations occurring in a carbonated atmosphere ('*Comptes Rendus*,' lxxxii, 1101).

The hæmoglobin of blood, according to P. Bert, absorbs more oxygen under increased pressure, this effect being less marked when the blood is in the body than when the experiment is made with freshly-drawn blood in an apparatus appropriate for securing the maximum of contact and of absorption between the blood and the oxygen (*ibid.*, lxxx, 733).

The physiological combustion or oxidation occurring within the body has been studied by E. Pflüger ('Pflüger's Archiv,' x, 251). His paper is chiefly a *résumé* and critique of many of the more recent researches into this subject; but it contains a theory of his own, with some original experiments suggested in its elaboration.

The influence of saline solutions on the coagulation of blood has been studied by A. Gautier ('Comptes Rendus,' lxxx, 1360). He finds that the blood of the animals with which he experimented—oxen, sheep, dogs, and rabbits—was best preserved from coagulation by a solution of common salt of such a strength as to contain not more than six parts of salt to 100 of blood, stronger solutions having less retarding influence. He further ascertained that many powerfully toxic salts, acids, and alkaloids do not prevent the coagulation of blood. From these and other results he concludes that the death of the blood is necessary for its coagulation.

According to E. Matthieu and V. Urbain ('J. Pharm. Chem.' (4), xx, 337), blood coagulates through the action of carbon dioxide. F. Glénard finds that arterial or venous blood, removed from the body in a segment of its containing vessel, does not coagulate, even though its moisture may have been greatly reduced, until it comes in contact with some foreign solution. But if evaporation through the vessel be prevented, then coagulation soon occurs. Glénard states that ox blood can be successfully transfused into the circulation seven hours after its removal from the animal, provided it remains fluid in its containing vessel ('Comptes Rendus,' lxxxi, 102). E. Matthieu and V. Urbain criticise the above view of F. Glénard, reaffirming their own theory that the absorption of carbon dioxide is the determining cause of the coagulation of blood (loc. cit. 535).

Chemistry of Milk.—Soxhlet has been continuing his inquiries into the constitution of milk ('Landw. Versuchs-Stationen,' xix, 118). Unfortunately this author is but imperfectly acquainted with previous researches, and is merely fighting a shadow when attempting to overthrow the theory which assigns a membranous envelope to every globule of fat in milk. No one, unless his knowledge of milk chemistry is twenty years old, believes in these envelopes of casein or other albuminoid substance. G. Bunge has made a large number of careful analyses of milk ('Zeitschr. für Biologie,' x, 295). He compares the amounts and ratios of sodium, potassium, and chlorine present in various milks with the corresponding figures obtained in the analysis of foods and of the bodies of sucking animals. In woman's milk the total solids amounted to 13.49 per cent., but not more than 1.55 per cent. of albuminoids was present, just about one third of the average percentage of flesh-formers in cows' milk. The ratio of the weights of soda to those of potash and of chlorine was variable in different examples of woman's milk, yet was less influenced than might have been anticipated by the administration or withholding of common salt. A ratio commonly observed approached one of soda (Na_2O) : three of potash (K_2O) : two of chlorine. The author points out the importance of the use of salt with certain, mainly vegetable, diets, such as those into

which potatoes largely enter, for these tubers contain more potash than any other staple food and require a larger addition of common salt. Rice, on the other hand, is stated to be the only vegetable product in which, without the addition of salt, man may subsist. Dr. C. A. Cameron ('Chem. News,' xxi, 54) announces as a discovery of his own that there is much suspended casein in normal cows' milk. This fact has been known for about twenty years (see 'Bouchardat et Quevenne "Du Lait"'). Cameron still adheres to the exploded "caseous envelope" theory. He gives some analyses of average normal cows' and mares' milk; these agree with the results now generally accepted fairly well.

Chemistry of Ferments.—Many chemical actions have been assigned to the intervention of definite chemical ferments not organised nor capable of increase or reproduction, like yeast and other undoubtedly organised and growing ferments. To distinguish these two kinds of ferments, Müntz proposes the use of chloroform ('Comptes Rendus,' lxxx, 1250), which paralyses and ultimately kills the latter, but has little or no effect in arresting the characteristic fermentations brought about by ptyalin and diastase in starch-paste, or by synaptase or amygdalin. E. Donath ('Deut. Chem. Ges. Ber.,' viii, 795) has worked somewhat in the same direction, having isolated a purely chemical and wholly unorganised ferment from yeast. This ferment is not a true albuminoid, though containing much nitrogen. Its transforming power has not been completely studied, but it is capable of inverting cane sugar most readily. Another chemical ferment has been isolated from the liver of rabbits by W. Epstein and J. Müller (ibid, viii, 679). It has the power of changing glycogen into sugar. It was removed from the carbolated and partially-dried livers by means of the solvent action of glycerine. W. K. Yasnopolsky has also isolated an unorganised ferment from the liver. O. Nasse has tried the influence of several saline solutions (four per cent.) upon the activity of some unorganised ferments (Pflüger's 'Archiv.,' xi, 138). He finds that the inverting chemical ferment from yeast, previously studied by E. Donath, is favoured in its action by ammonium sulphate, and much hindered by potassium chloride. Other allied ferments are differently affected by the above and by other salts.

Chemistry of Urine.—Some investigations on the production of so-called conjugated sulphuric acids in the organism have been made by E. Baumann (Pflüger's 'Archiv.,' xiii, 285). He finds phenol-sulphate of potassium in the urine of patients to whom carbolic acid has been given either internally or externally. This salt being non-poisonous, he suggests the administration of sulphate of soda as a chemical antidote in cases of poisoning by carbolic acid. Musculus and de Merme ('Comptes Rendus,' lxxx, 958) have discovered what seems to be a new acid in the urine of patients taking chloral. It contains 31.6 per cent. carbon, 4.36 per cent. hydrogen, and 26.7 per cent. chlorine. It is, when pure, a crystallisable acid, having very distinct properties; it does not yield chloroform, and its abundant formation in the body further negatives the older

theory of the physiological action of chloral. E. Baumann has succeeded in isolating a conjugated sulphuric acid, or rather a sulphonie acid, from urine ('Deut. Chem. Ges. Ber.' ix, 54). The potassium salt of this acid may be obtained in distinct crystals, which, when dissolved in water, show a blue fluorescence. This salt is potassium phenol-sulphonate, and splits up under the influence of strong mineral acids into carbolic acid and potassium sulphate. The same experimentalist has ascertained more recently (Pflüger's 'Archiv,' xii, 69) that, although all the sulphuric acid existing as sulphates may have been removed from a sample of urine, treatment of the prepared liquor with heated acids generates a quantity of sulphuric acid by reason of this decomposition of the sulphonates present. J. Munk has shown that a vegetable diet, and also the administration of benzol, increases the quantity of the phenol-sulphonate in human urine (loc. cit., 142).

Two of the reactions of glucose were observed with a sample of urine from a patient supposed to be suffering from diabetes. Caustic potash produced a brown colour on heating, while Fehling's sugar test gave a red precipitate of cuprous oxide, though long boiling of the liquid was necessary to produce this effect. The substance thus simulating the reactions of a sugar was not isolated in a state of purity, but it was found to be without action on polarised light (M. David, in 'Bulletin Soc. Chim.' (2), xxiii, 235). E. Reichardt has found a substance resembling dextrin in the urine of persons suffering from diabetes (Archives 'Pharm.' (3), 502).

W. von Knieriem has ascertained that nine tenths of the nitrogen of sal-ammoniac administered internally reappears in the urine as urea, the remainder chiefly as ammonium compounds. Both aspartic acid and asparagine also originate much urea ('Zeitschrift f. Biologie,' x, 263). M. Nencki has been investigating the several pigmentary bodies related to indol and indigo which appear in the urine. He connects the production of indol with the pancreatic digestion of albumen ('Deut. Chem. Gesell. Ber.,' vii, 1593). F. Baumstark (Pflüger's 'Archiv,' ix, 568) has extracted two colouring matters (of doubtful purity) from the urine of a patient suffering from splenic disease. He employed a process of dialysis.

Sarcosine, a derivative of creatine, has been found by E. Baumann, J. V. Meering, and E. Salkowski to pass out unchanged, in part at least, when given to dogs ('Deut. Chem. Ges. Ber.,' viii, 581, 638).

Chemistry of Poisons.—In connection with the subject of the toxic characters of fusel-oil, we have some experiments of Dujardin-Beaumetz and Audigé ('Comptes Rendus,' lxxxiii, 80). The trials were made with dogs, the fatal doses being determined for five different alcohols, and by two methods—administration by the stomach, and by hypodermic injection. For each kilogramme of live weight the quantities necessary to produce death were these: Of methyl alcohol, 5 grains; of ethyl alcohol, 7.75 grains; of propyl alcohol, 3.13 grains; of butyl alcohol, 1.74 grains; and of amyl alcohol, 1.48 grains. Thus it appears that the most abundant con-

stituent of fusel oil is the most poisonous, and that this amyl alcohol is five times as powerful as ordinary alcohol. And it further seems that the dilution of some of these higher alcohols of fusel oil with ordinary alcohol enhances greatly their toxic effects.

Dr. T. L. Brunton and Sir J. Fayrer have continued their researches on the physiological action of the poisons of various snakes ('Proc. Roy. Soc.,' xxiii, 261). Professors McKendrick and Dewar have continued their experimental inquiries into the physiological action of the chinoline and pyridine bases (loc. cit. 291).

REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIENE.

By BENJAMIN W. RICHARDSON, M.D., F.R.S.

I. TOXICOLOGY.

Poisoning by Coal-gas.—We are indebted to Dr. Gilbert Smith for the following admirable report on this subject.

"On the morning of the 26th October, 1876, I was summoned to see a patient of Mr. Roberts of Southgate, who was suffering from the effects of inhalation of coal-gas.

"On arriving about 11 a.m., I found a young gentleman of the age of 17, in bed in a state of insensibility. From inquiry I learned that when calling him at the usual hour the servant on receiving no reply entered the room and perceived a strong smell of gas, which was escaping from the gas jet. Being unable to arouse the patient Mr. Roberts was at once sent for, and he describes his appearance as follows :

"When first called to the patient I found him in bed insensible; the surface cold with rigid contraction of the muscles generally, but more especially the flexors, the elbows being flexed and the hands tightly clenched. I could feel no pulse at the wrist, but on placing my hand over the heart the action could be felt—feeble, irregular, and fluttering, but quite impossible to count. The pupils were contracted and utterly insensible, and the first sign I saw of improvement was after about three quarters of an hour, when they became more dilated, and on exposure to light slightly contracted. The breathing was neither rapid nor jerking."

He was removed into another room, and Mr. Roberts ordered mustard poultices to be applied to the nape of the neck and hot water bottles to his feet. He endeavoured to administer brandy and water but could not produce deglutition, nothing seeming to rouse the patient from the profound coma. Between ten and eleven o'clock he broke out into a profuse perspiration, when the temperature was found to be 103°.

Shortly after this I saw him and found him a well-nourished,

strong, and muscular young fellow. He was lying on his back with pallid face and closed eyes, completely unconscious, and making no response, even when loudly and sharply addressed. His skin was moist and chilly, of a peculiar earthy colour, and was insensible to pinching, &c. There were no spots observed. The arms were flexed across the chest, the fingers bent, while the whole muscular system was rigid, but continually twitched with slight convulsive movements. The muscles of the lower jaw seemed especially irritable, there was occasional grinding of the teeth, and once on trying to depress the jaw it resisted and became rigidly fixed. There was no foaming of the mouth, although the saliva was abundant. The lips were a bright red, as also was the conjunctival lining of the eyelids.

The eyes had a glistening appearance, were not turned upward, and when they moved did so unsteadily. The pupils were contracted equally, and slowly responded to light. The pulse was beating 140 per minute; it was thread-like and uneven, and at one time after raising him I fancied I detected some irregularity. The impulse of the heart was weak and hardly to be felt, and the rhythm of its sounds was uneven. Respiration was being fairly sustained, the chest expanding well, but although much improved it had a marked tendency to the ascending and descending characteristics of the "Cheyne-Stokes" respiration. The condition of the abdomen was normal, with the exception of the occasional rigidity of its muscular walls, and that the bladder could be felt distended above the pubes.

The treatment was simple—pure air, warmth, and stimulant in hot strong coffee. The doors and windows were opened so as to yield a thorough supply of fresh air, while he was well covered up and kept warm, and with much difficulty and loud commands we were able to get him to swallow some strong black coffee with a tablespoonful of brandy. Another mustard poultice was placed over the heart, and in a short time we were delighted to notice signs of returning consciousness, such as opening the eyes, and natural movements of the arms. His pulse was less rapid and more even, and his colour altogether more lifelike. About half-past twelve we again, this time with ease, got him to take more coffee and brandy. On calling him by name he slowly answered, and on asking him if he suffered pain and where, he replied, "Yes, here," putting his hand to his eyes and forehead. From this time he gradually improved, the insensibility slowly giving place to returning consciousness. Mr. Roberts had tried to draw off the retained urine by the catheter, but failed owing to the strong contraction of the sphincter. This state with the muscular rigidity lasted much longer than the insensibility, and when the brain became clear the retention of the water gave great uneasiness, so that, after repeated painful and unsuccessful attempts to micturate, he was placed in a warm bath and the urine thus drawn off. The hot water jar applied to the feet had, of its own accord and unbidden, produced a large blister on one foot, which doubtless was of use in awakening consciousness. His recovery from the serious and at one time

almost hopeless state was perfect, and was followed by no organic mischief or ill consequences whatever.

Unfortunately no examination of the urine was made at the time, but some ten days afterwards it was found normal in quality and quantity, no albumen or sugar being present.

Cases such as these possess some considerable clinical interest, and the more we know of their pathology and treatment the better able we shall be to perform our duty at the bedside of a similar case. To this instance also was attached a forensic aspect, for the question of course arose at once as to how came the gas to be turned on, for he slept in a room by himself and no one was known to have entered. The previous history and disposition of the boy along with a thorough inquiry resulting in no evidence as to cause, at once negated the idea of wilful intention, and a decided testimony of his habit of sleep-walking pointed clearly to the manner and origin of the accident. Some may think that such an origin is unlikely and improbable, but I was helped to accept it with ease owing to my knowledge of a similar case in which a schoolboy was seen, by his companions who watched him with great interest, deliberately to rise from his bed in his sleep and light the dormitory gas by striking a match. These facts should be a caution to somnambulists and their friends; they add to the already well-known dangers of having gas in bedrooms, and clearly point to the greater security of those who make it their practice to have it nightly turned off at the meter.

The interesting clinical feature in the above case is the condition of the pupil; in all those cases which I have read the pupils were dilated, some extremely so, but here it was intensely contracted and insensible to light, and its improved condition was the precursor of a general improvement. The spasm also of the sphincter of the bladder lasting so long as it did points to the irritant as well as the narcotic properties of the poison, and explains the four cases of M. Tourdes, in which he found on post-mortem the bladder filled with an enormous quantity of limpid urine. The change for the better was ushered in by a sort of crisis, a sudden acceleration of temperature and perspiration which at first was thought to betoken a change for the worse, but not so, the dilatations of the arterioles and blood-vessels of the skin and extremities not only relieved the central pressure, but gave the sudoriferous glands an opportunity of removing some of the poison from the system.

In the present day when coal-gas is so universally used, and where as I am informed, a tenth part of that manufactured is unaccounted for, and therefore—escaping anywhere or everywhere—is not unlikely to do mischief, it is well for the medical practitioner to be prepared to meet face to face, and to treat successfully, a case of poisoning from its effects. To do this he must have some knowledge of its nature and composition, he must not be ignorant of its physiological action, nor of the various means used to produce recovery. Coal-gas is a compound gas, its ingredients varying considerably in their relative proportions according to the kind of coal used and the method of its manufacture. According to Dr. Tidy, it is composed

of the following gases:—1. $\frac{1}{2}$ Hydrogen; 2. Olefiant gas and other heavy hydrocarbons; 3. Light carburetted hydrogen; 4. Carbonic acid; 5. Sulphuretted hydrogen; 6. Ammonia; 7. Oxygen; 8. Nitrogen.

Of these, some agree with M. Tourdes in considering that the carbonic oxide is the *only* active poison; and undoubtedly it is generally acknowledged to be the most active of any. It is present to the extent of 5 to 7 per cent. in the gas supplied to London. A small quantity of carbonic oxide in the air is sufficient to kill; according to the experiments of Leblanc and Dumas, one per cent. will kill a dog in one and a half minutes, and birds immediately die in an atmosphere containing five per cent.; animals so killed revealed after death florid blood, congested brains and auricles gorged. Dr. Letheby invariably found effusion of blood in the brain of birds. Nysten found that injected into the veins it gave a brownish tint to the blood, and induced a state resembling intoxication. When Sir H. Davy inhaled it, giddiness, sickness and prostration were the results. Mr. Higgins, of Dublin, records in his interesting and daring experiments that one of his assistants who inhaled three or four deep inspirations after previously exhausting the lungs, was suddenly deprived of sense and motion, fell down supine, and continued for half an hour insensible, apparently lifeless, with the pulse nearly extinct. Various means were tried for rousing him without success; till at last oxygen gas was blown into his lungs. Animation then returned rapidly; but he was affected for the rest of the day with convulsions, agitation of the body, stupor, violent headache and quick irregular pulse; and after his senses were quite restored he suffered from giddiness, blindness, nausea, alternate heats and chills, succeeded by feverish broken but irresistible sleep ('Edin. Philos. Journal'). Carbonic oxide gas is undoubtedly a narcotic poison possessing strong nerve irritant properties, its action is believed to be due to the formation in the blood of a new compound of carbonic oxide with the hæmoglobin of the blood. The blood is hyper-arterialised or as Bernard says, death follows because the blood cannot become *venous*, the opposite condition to that obtained in death from carbonic acid where the blood cannot become *arterial*.

It is not yet known what part the hydrocarbons play in coal gas poisoning, but Dr. Taylor has no doubt that they have a noxious influence. The percentage of light carburetted hydrogen present in London gas is from 40 to 45. It is a narcotic poison, but not so energetic as sulphuretted hydrogen. When Sir H. Davy (making *himself* the medium of experiment) breathed it pure, "the first inspiration caused a sense of numbness in the muscles of the chest, the second an overpowering sense of oppression in the breast, with insensibility to external objects; during the third he seemed sinking to annihilation and the mouthpiece dropped from his hand. On becoming sensible, which happened in less than a minute, he continued for some time to suffer from a feeling of impending suffocation, extreme exhaustion and great feebleness of pulse. For the rest of the day he was affected with weakness, giddiness and rending headache." Yet that this gas when mixed with air even

in an explosive proportion can be respired with comfort is proved by those who, protected by Davy's lamp, toil daily in our coal mines. It is, then, to these two formidable gaseous poisons that coal-gas chiefly owes its dangerous properties. They are very similar in their action on the nervous centres and on the heart, but the effect of the carburetted hydrogen is transient compared to the more lasting and more alarming consequences of the carbonic oxide. The varieties of the symptoms and the difference in the post-mortem appearances depend, not only on the gas itself being a varying compound, but also on the form of its administration, whether pure, highly concentrated, largely diluted with air, or whether the exposure to its influence was long or short. The experiments reported in the forty-fifth volume of the 'Transactions of the Royal Med. Chir. Soc.' by Dr. Aldis demonstrate this:

They consist of two series; the first, in which rats were immersed in an atmosphere of pure coal-gas, resulted as follows:—

Almost immediate gasping, insensibility in from twenty to thirty seconds; spasmodic action of the diaphragm and death, in three out of the five animals experimented on without any convulsive movement, in from one to a minute and a half. The eyes projected greatly and were open, the outer surface of the skull was exceedingly red, and dark fluid blood escaped from the heads when opened, the superficial vessels on the surface of the brain pinkish and some empty, the substance of the brain pale. The lungs collapsed and of a pink colour, the heart distended with *darkish* fluid blood and congestion of the abdominal veins. The brains of all in this series were free from congestion.

The second series was made with gas diluted with atmospheric air in different proportions. Dr. Aldis describes the effect on a rat placed in an atmosphere containing one-fifteenth of common gas as follows: "It soon began to pant very fast, the head fell with inclination of the body to the right side in ten minutes, the head was subsequently raised and fell, which was followed by two springs of the body; sharp twitches of the body and left ear took place at the end of seventeen minutes, when the animal seemed to be insensible, with the eyes open. We observed a repetition of these symptoms in twenty-two minutes, and again in half-an-hour and three minutes. In forty minutes the respiration became laborious, when death occurred, twenty-five cubic feet of the mixture having been used. The eyes were open, the surface of the skull intensely red, the brain congested, the blood being fluid and *light coloured*. The plura of a bright red colour and the right side of the heart distended with darkish fluid blood; no redness in the trachea."

The appearances found in the first series correspond with those found in the published cases of Mr. Bloxam and others, where the poison was concentrated and death rapid, and seem to be due to asphyxia, and to exhibit the usual phenomena of asphyxia in the dissection. But in the second series of Dr. Aldis' experiments, and in recorded cases of prolonged exposure to diluted gas, there was sufficient time allowed as it were for the specific action of the poison

and a deferred death enabled dissection to reveal the phenomena of poisoning from the carbonic oxide and the hydrocarbons.

As to treatment we are still very ignorant except on a few obvious points. Exposure to fresh air and warmth to the surface is alone sufficient in many cases. Artificial respiration, where necessary, has been used with success. Cold effusions to the head, ammonia rubbed over the chest, turpentine enema, have all been tried with varying results. Galvanism was used in an interesting case of Dr. Babington's, but which he abandoned owing to the excitement being followed by a corresponding depression. Oxygen has frequently been administered pure, its utility is a disputed point; the animals in Dr. Aldis' experiments recovered more quickly when exposed to fresh air than when either oxygen or ammonia was adopted. Those who hold that the union of carbonic oxide with hæmoglobin forms a fixed compound, think that it is harmful, and that transfusion of arterial blood is the only chance of saving the patient; but Donders attempts to prove that oxygen expels the carbonic oxide, and his view certainly is supported by Mr. Higgins' experiment. Dr. Richardson strongly holds that alcoholic stimulants fix the carbonic oxide in the blood and increase the evil still more. In my case the hot strong coffee certainly seemed quickly beneficial and the poultice to the cardiac region had at once a good effect."—*Original Report by Gilbert Smith, M.A., M.D., December, 1876.*

Poisoning by Carbonic Acid, Carbonic Oxide, and Coal Gases.—In connection with the facts and opinions recorded above by Dr. Gilbert Smith, the following additional facts by Dr. Robert Harvey, of the Bengal Army, may be read with interest. Dr. Harvey shows that deaths from these poisons are becoming frequent and are likely to become more frequent, year by year, in India as the railways are extended and the coal fields are developed. In the returns from which the author is reporting as many as fourteen cases of poisoning by the gases named are recorded.

Four of the fourteen cases, which involve twenty-five individuals, occurred in 1870, three in 1871, and seven in 1872. Two are from Raniganj; three from Buxar; and one each from Bardwan, Budaon, Gen-dah, Lucknow, Mussuri, Shaharanpur Jalandhar, Rurki, and Delhi. All but two occurred in November, December or January, when the cold is often intense and the thin clad natives close up every cranny in their houses to keep it out. Nine of the cases were due to—

(a.) Gases derived from the burning of fuel, and all but two occurred in masonry buildings, belonging to railway or coal companies. The ordinary hut of a native is hardly ever air-tight, and in spite of his efforts to close it, almost always has enough vents to prevent the noxious gases given off by the charcoal from accumulating in fatal quantity. If it were not so, nothing would be commoner than carbonic acid poisoning, for it is a very general custom to keep a pan of charcoal burning at night during the cold weather. The small out-building attached to railway stations on the other hand are for the most part of solid masonry, and in many cases have no outlet for air, but a closely-fitting door. This the ignorant

native closes, and going to sleep as near the fire as may be, is generally unable to escape, even should he wake to a consciousness of his danger.

The following well-reported case by Dr. Warburton of Jalandhar is a good example of poisoning by charcoal vapour:—

“On the morning of the 29th December, 1870, a Babu named Tara Chand Khutree, aged 22, in the employ of the S. P. and Delhi Railway Company at Phillour, and his servant Kamda, a Brahmin, aged 40, were found dead in one of the servant’s houses in the station compound at Phillour. The previous evening had been cold, and the servant on retiring to rest had placed a pan of live charcoal under the head of his Charpoy, and had closed the door, which appears to have been the only aperture for ventilation. In the morning this charcoal was found consumed, and of the clothes and bedding of Kamda only a few shreds remained, while those of his master were untouched. I examined the bodies about noon on the 30th, and *post-mortem* appearances were as follows:—

“*Tara Chand*.—Body well nourished; bloody mucus flowing from mouth and nostrils in small quantity. Face somewhat livid. Eyes—conjunctivæ congested; pupils slightly dilated, covered with a film; strong smell of charcoal from body and clothes; teeth firmly clenched; tongue firmly pressed against teeth. Cadaveric rigidity strongly marked. Abdomen distended with wind, and the body extensively marked with livid spots. Fingers bent, though not actually clenched. Scalp very much congested, membranes of brain injected with dark blood, sinuses distended with blood. Slight amount of serum beneath arachnoid, a small amount of pinkish serum in left lateral ventricle, right full. The choroid plexus of a dark purple colour, and a considerable amount of serum at base of brain; many red points in brain substance. Lungs and lining membrane of trachea much congested. Right side of heart and whole venous system gorged with dark fluid blood, left side empty; valves healthy. Stomach contained a lot of gas and undigested food; mucous membrane throughout of a pinkish colour, healthy. Livers, spleen, and kidneys healthy.”

The appearances in the servant were similar, but were somewhat modified by the fact that his clothes had caught fire, inflicting severe burns “which from the absence of vesicles or unusual redness around the burnt parts appeared to have had effect after death.”

Mr. E. J. Roberts, M.B., gives a very similar case in the *Raniganj* return for November, 1871, where a man and his wife were killed in a pakka out-house belonging to the Bengal Coal Company by the vapour from a pan of cinders. Mr. Roberts remarks:—

“Eleven deaths from this cause have now come under my notice since the 4th December, 1869, all of them in pakka brick buildings, in which the deceased had gone to sleep leaving a fire burning, after closing all the openings to exclude the cold night air. A twelfth (a child) was found still alive, and recovered. A European and his wife, who had gone to sleep after closing the glass doors of their room, and leaving a pan of cinders burning on the floor in a rail-

way bungalow at Pakour, in December, 1870, were fortunately discovered in time to save their lives. The man telegraphed that they were 'affected with suffocation, severe throbbing in head and faintness, with sick feeling; can scarcely move.' "

In another case, where the wife and two children of a pointsman of the East India Railway were poisoned by coal vapour in a small house 8' x 8' x 7' high, the two windows of which had been carefully filled in with bricks and mud, Mr. Roberts remarks that "natives are evidently not aware of the dangers of shutting themselves up in closed pakka houses with a fire burning." It is hardly to be expected that they should be, and the practical lesson to be derived from these cases is that the railway companies should always provide efficient ventilation in their huts, and make some responsible person see that windows and other outlets are not bricked up by the occupants. The following interesting case, where a rescue was effected in time, is by Dr. R. Temple Wright, in medical charge of the Buxar stud.

"On 12th January, 1872, my railway native doctor called my attention to a patient just brought to him insensible, but without any explanation of how he became "behosh." I employed artificial respiration (according to Sylvester), when he gradually revived, and I asked where he was found. The night assistant station-master said he would show me, and led me to a small pakka hut in which the patient, a pointsman, was supposed to take shelter when on duty only. As it was on a line where waggons were shunted to the goods-shed, it was not used at night, so a pointsman chose to sleep there. On this occasion he did not come to relieve his predecessor on duty, so the latter (becoming impatient at being kept so long waiting in a cold night of January) fortunately went to call him. Receiving no answer, he dragged him out and kicked him, but as this failed to rouse him, he brought him to hospital, where he revived, as above described, and thus fortunately escaped registry in my police case book."

The success attending the artificial respiration is encouraging, and should be kept in mind. In a case reported in January, 1870, by Dr. Carmichael, then in charge of the Buxar stud, a woman and child found dead in a pointsman's house were sent in for examination as cases of snake-bite. There was nothing to indicate snake-bite, and Dr. Wright remarks that snakes are not seen in Buxar during the cold weather, and are at any time rare on a railway where the ground is rough and trains are always passing. Dr. Carmichael visited the hut and brought out very clearly the circumstances of the case. The *post-mortem* appearances were similar to those in the cases already given, and the woman had been much burnt about the head.

"On the 28th January, 1871, a male Ahir, aged 40, was brought to the Buxar dispensary 'covered with burns from head to foot.' It had appeared that in the previous night he and his family (wife and four children) retired to rest, with a pot of red hot charcoal inside the house. Towards morning the neighbours broke into his house calling him to come out as it was in flames. He was sleep-

ing with his head near the door, so this most likely saved his life, for he was not so completely suffocated that he did not wake when his clothes caught fire and the neighbours dragged him out. Just after his rescue the whole of the house fell in, and a tremendous blaze consumed everything within. His wife and children must have been quite stifled already, for no sound was heard, and only a few charred remains were found when the fire died out. Though his burns were so extensive they were quite superficial. He made a capital recovery, and was discharged cured on the 2nd March, without any contraction of cuticle, and indeed almost without a scar."

Three men at Massuri, in November 1872, went to sleep in a small apartment with a charcoal fire in it. When found, 36 hours after, two were dead, the third insensible. The latter was semi-comatose when seen by Surgeon-Major Odevaine. The *post-mortem* appearances on the patient's dead comrades were congestion of the lungs, with froth in the bronchi. One had in addition great congestion of the brain, with apoplectic effusion on its surface, and serum in the ventricles and at the base. The livers were congested and dark fluid blood was found in the hearts, the right side in one and both sides in the other being full. The remaining cases call for no special remark.

b.—Carbonic Acid and Oxide, developed in an old grain pit. Mr. A. Eteson gives a case in the Rurki return for December, 1871, where a Jatni, aged 20, was thrown down a grain pit which had been emptied about a month previously. Immediately after her fall she complained loudly of being very giddy and of a sensation of suffocation, but became silent after a few minutes. Help not being at hand, she was not rescued for seven hours, when she was brought up dead. The *post-mortem* appearances are thus described:—

"No marks of external violence on the body; eyes highly congested and prominent; neck, face, and head swollen; face livid; jaws closed and tongue firmly pressed against them; abdomen inflated. *Head*.—The brain and its membranes congested. A small quantity of serum, slightly tinged with blood, was found in the ventricles. *Heart*.—Right side distended with fluid blood. Left side was almost empty. Spleen, liver, bladder, uterus, and kidneys, were found healthy.

"Stomach and intestines were distended, almost to bursting, by gaseous matter."—*Indian Medical Gazette, June 6th, 1876.*

[Two subjects of special interest occur to the mind on reading the narrative supplied by Dr. Harvey. The first relates to the difference in the condition of the blood in the cases of poisoning by carbonic oxide, when his cases are compared with those to which Dr. Gilbert Smith has directed attention in his very able report. The blood, it will be seen, was of dark colour on both sides of the circulation in all the Indian cases of poisoning by carbonic oxide, instead of being bright red on both sides of the circulation, as we observe it to be in this country from the same cause. The anomaly is of greatest importance in a medico-legal point of view, and the reason of it requires to be made clear. It may possibly be that the difference of colour of the blood, produced by carbonic oxide in India, is due to

the difference of temperature, to the greater heat of the climate of India, as compared with that of the temperate regions. Dr. John Davy showed that in the tropics the difference of colour between venous and arterial blood is almost imperceptible, and in the heat of summer in this country we have observed the same fact. It is possible, therefore, that in a raised temperature the effect of carbonic oxide, in reddening the venous blood to arterial hue, does not follow the administration of the gas. It is important for our Indian brethren to test this suggestion by direct experiment. We have taught here, from the results of experimental and general observation, that redness of the venous blood is one significant sign, at least, of poisoning by carbonic oxide. But if climatic difference modifies this experience, the sooner the modification is proved, the better for the welfare of the community.

The second subject of special interest in the reports named, relates to the distension of the body with gases, after poisoning by carbonic acid and carbonic oxide. We believe this to be a common phenomenon after gaseous poisoning. It is as though the gases absorbed by the lungs were eliminated direct into the alimentary canal, and the same applies to the elimination of vapours, such as ether, methylene, and chloroform. In inspecting a dead body supposed to have been poisoned by inhalation, it would be a good practice to collect the gases included in the canal for the purpose of analysis.—B. W. R.]

II.—FORENSIC MEDICINE.

Death from heat Tetanus.—We are much indebted to Staff-Surgeon Dr. Magill, of the Royal Dockyard, Sheerness, for the following valuable report.—“Charles Allen, aged 60, a stout muscular man, was employed on the 10th of October, 1876, in Sheerness Dockyard, attending to the melting of pitch in an iron pot which was swung from the arm of a crane over a strong, clear fire, in a movable bogie, standing in an open fireplace. He had been employed in the same occupation during the last thirty years. The pot and crane and empty fireplace are similar to what may be seen in country houses, where there is neither grate nor stove. The bogie is a metal vessel, perforated with holes, such as plumbers use, but larger, being 18 inches long, 12 broad, and 12 deep.

“A fellow-workman left Allen in his usual health and spirits, attending to the pot and fire at 12.45 o’clock, and after being absent exactly half an hour at his dinner, returned and found the crane and pot swung round to one side from the fire, and Allen enveloped in flames four feet high, lying face downwards, with his chest fairly on the bogie, his arms on each side of it, and his head and neck beyond it, against the back of the fireplace.

“It was difficult to reconcile the position of the body as found on the bogie, with either accident or a struggle to escape from the fire.

“The body was immediately pulled from the fire, and the flames extinguished with water.

“The Fleet-surgeon of the dockyard was near the spot, and saw the body within five minutes from the first alarm. Life was, of

course, extinct, but what struck him as well as the men who pulled the body off the fire, was its remarkable rigidity. When raised or turned, it moved as one solid, rigid mass, neither head nor extremities changing their relative positions when left entirely unsupported.

"All who first saw the body are unanimous as to the fact of its general rigidity, and the fixed position of the limbs when first seen by them. The body was placed on a broad wooden stretcher for removal, and on this I saw it lying. The position was similar to that of a person lying down on his face, and 'throwing his heels in the air.' The clothes were almost entirely consumed, but the parts of the body most deeply burned were the upper half of the chest, the arms, and the fronts of both thighs; in these places the skin was completely destroyed, and the muscles exposed, standing out strongly contracted. The head was drawn slightly forward, the jaws firmly closed, with the tongue caught between the teeth. The left arm was close to the side, with the elbow and wrist-joints flexed, so as to bring the hand near the axilla. The right arm was extended from the side, with the hands brought close to the side of the head. The phalangeal joints of all the fingers were strongly flexed, while the metacarpophalangeal joints were just the contrary, so that the hands were broadly open, while the fingers were clawed.

"The thighs were partially flexed and apart; the right one most flexed and abducted. The knee-joints were flexed to quite right angles with the thighs, so that the soles of the feet looked upwards. The right leg was most flexed, and one of the men told me he had noticed *this leg growing more flexed* while he was extinguishing the flames—a fact of importance, as indicating the *post-mortem* nature of the rigid contraction of the muscles. The penis, also, was completely retracted, its position being marked only by a series of concentric folds of integument at the upper part of the scrotum, like what is seen in a case of large scrotal hernia.

"Twenty-four hours after death the rigidity had not diminished, and a coffin had to be made large enough to contain the body with the limbs still in the position in which they were found.

"The point of peculiar interest in this case, and that for which alone I have brought it under notice, is the state of rigidity in which the body was found to be even while still in flames, and before life could have been extinct many minutes. It was suggested as a possible explanation, that the man may have had a violent fit, and falling on the fire, was smothered or burned to death suddenly before the fit was off. But there was no symmetry in the positions of the limbs as in the contractions of a fit, and death generally relaxes spasm. Besides, a son of deceased gave evidence at the inquest that 'his father never had fits.' It, therefore, appears to me more reasonable to consider the rigidity as true rigor mortis, produced by the rapid coagulation of the muscle fibrine through the whole body by the intense heat. This explanation agrees with Dr. Richardson's views of rigor mortis, as given in an article on that subject in the 'Transactions of the St. Andrew's Med. Graduates' Association,' vol. i, in which he says that 'rigor mortis will always

be more quickly developed when the temperature of the day or night is high; and 'the cause of rigor mortis is coagulation of the muscle fibrine, and the excitant of the coagulation is heat.'"—*Original Report, by Dr. A. Magill, M.D., F.R.C.S., Staff-Surgeon, R.N.*

[Dr. Magill has here recorded the first definite case of instant rigidity from heat in the human subject. The phenomena discovered are so absolutely in accord with the facts which have been observed in physiological research, there is no occasion whatever to assume the occurrence of any kind of convulsive seizure to account for them.—B. W. R.]

III.—HYGIENE.

Ventilation of Sewers.—The Metropolitan Board of Works reports on the ventilation of sewers and the recommendations contain many notes of considerable value. The report gives a chronological sketch of the progress of inquiry since the Vestry of St. Marylebone forwarded the report of their medical officer on the subject to the Board of Works in 1856. The most important conclusions embodied in the present official paper are as follows:—

Some provision for the ventilation of sewers is absolutely necessary, both for the safety of the men engaged upon, cleansing, repairing, and other necessary works in the sewers, and for the stability of the sewers themselves in the event of an explosion.

The method of ventilation adopted in mines where there are only two openings to be dealt with (an inlet for the air at one end and an outlet for it at the other) is inapplicable to sewers.

The most efficacious and widely applicable mode of preventing the escape of offensive effluvia from the sewers is to provide them with such a copious supply of water that the decomposing matter within them shall be diluted and removed before any noxious gases have been generated.

Ventilation by air-shafts from the sewers into the centres of the streets is a great improvement upon the old system of ventilation through the gully gratings at the sides of the streets.

It is inexpedient to incur a large expenditure for the purpose of making experiments to determine the value of the plan of ventilating sewers by means of furnaces.—*Report on Ventilation of Sewers, July 21st, 1876.*

Biometry.—Under the term biometry, Dr. Moreau Morris, of New York, discusses the different methods of measuring the value of life in different individuals. He dwells on the fact that longevity is a resultant of heredity, and that those who have inherited it can, seemingly with impunity, almost defy all sanitary law, and yet continue to live up to and beyond the allotted limit of threescore and ten: while those who have not inherited long life cannot, by any system of life, or observance of the laws of health or processes of prolongation, protract their naturally short-lived inheritance. One organ of the body essential to life may die out before the others, owing to hereditary shortness of life of that organ. In the study of biometry we proceed, says the author, to fix upon such

indications as are found to pertain uniformly to the long-lived. These he includes under three heads, *size, form, colour*, with texture or quality of tissue. Passing over the details relating to size and form, we may add one or two in which reference is made to colour. On this head the author remarks, that "with some exceptions we find the rule to be, that sound, vigorous health is indicated by a roseate-tinted skin, showing the circulation of blood possessing normal constituents in due proportions, such as only a good physiological organism can furnish.

"Ill health, systematic obstructions, congested capillaries, or biliary derangements produce general discoloration, and may be looked upon as indications of grave import, while local changes may only indicate transient ailment or injury of the special part. A pale, waxy cuticle, if accompanied with œdema, and congested capillary vessels about the nasal alæ, usually indicates renal disturbance, which may often be diagnosed even before the presence of albumen is detected in the urine.

"The colours of the iris are, strictly speaking, either blue or brown, presenting every variety of shade, whose perfect admixture produces the true hazel. The colour of the iris never changes in the individual, except as to shades, and presents one of the strongest indications of the original fundamental constitution. Thus the two temperaments, bilious and sanguine, are well represented; the first by the brown, the second by the blue; the former, venous, sluggish; the latter, arterial, quick. In the pure hazel we find these two grand temperaments well blended, nearly always associated with vigorous, healthy long life.

"Either of these colours may be found in aged persons, however, as may be observed in some of the different races; for instance, the black hair, dark brown eyes, dark bilious complexion, and dark beard of the Jewish race, or the light blue eyes, Saxon hair, and fair complexion of the German and Scotch races. As a rule, when the iris looks watery, weak, colour thin or transparent, either brown or blue, there may be a suspicion of defective constitution; while a pure, deep bright colour is a strong indication of vigorous life. Very dark hair and beard, with light eyes and pale skin, show scrofulous and consumptive tendencies.

"The colours of the hair, beard, eyes, and skin compared, will then present good indications of health and disease, as well as of personal and ancestral longevity. Dark red hair and beard indicate nearly the same bilious temperament as dark brown or black hair with brown eyes, and with the same tendencies to diseases of the liver and digestory organs, and their usually attendant consequences, piles, rheumatic and systemic affections. When the eyes are of a reddish-brown tint, easily lighted up under excitement, caused by a more rapid circulation, we have the most decided indication of a tendency to apoplexy, liability to sunstroke or sudden cerebral congestion, and, as in these temperaments there is usually less muscular tenacity of blood-vessel, it is with such that the smaller blood-vessels give way under pressure from undue excitement or continuous mental activity.

It has also been remarked that comparatively few persons with pure brown eyes can be found living above 70 years of age. Usually they do not live beyond 60 to 65. On the other hand, it is rare to find persons over the age of 70 who have not pure hazel eyes."—*Biometry in relation to the Practice of Medicine. By Moreau Morris, M.D. Pamphlet, 1876.*

Death Rates in England and in Michigan.—Dr. Henry B. Baker, Secretary of the State Board of Health, Michigan, contributes another of his admirable essays on public health in his report on the death rate of each sex in Michigan, in comparison with Dr. Farr's life tables of healthy districts of England. To illustrate his subject, he has drawn out two diagrams, exhibiting the death rate in 1000 persons of each sex at each age in Michigan, and in the healthy districts of England. The first diagram proves that in these four classes of persons the death rate presents a very close resemblance at all ages until that of eighty years is reached, when the lines for England fall below those for Michigan, indicating, according to the data used, a higher death rate at the older ages in England than in Michigan. The slightly greater death rate in Michigan of those under one year of age may be explained by the fact that in Michigan still-births are included; but this will not explain the slightly greater death rate at other ages under five years.

The second diagram exhibits graphically the probable duration of life of males in Michigan; and by means of lines marking the upper limit of the probable life of females in Michigan, and of males in the "healthy districts" of England, the close correspondence therewith is rendered apparent. The starting-point for each sex in each country is the same as for the males in Michigan, namely, opposite each particular year of age, shown on the left of the diagram, for every decennial year.

One of the objects in view in preparing diagram No. 2 was to enable those who examine it most easily to appreciate the very great difference between the "probable life" and the "possible life" at each and every age. This diagram relates to the "healthy districts" of England, and to the healthful state of Michigan, and it is seen by the line referring to Michigan, in this diagram, that except for the ages between one and twenty-five years, man does not live out one half of his "possible" time; and of the males in England only those aged under thirty-six years live out one half of their "possible time." The total death rate appears to be about the same in the two localities. The difference in this statement in the ages, twenty-five in Michigan, and thirty-six years in England, is caused by the shorter possible time in England, the time of which is stated as 105, whereas in Michigan it is believed to be 112 years.

In addition to the above, Dr. Baker gives a paper on infant mortality in the State of Michigan, in which he shows that the causes contributing most to infant mortality are in the order named:—viz. Scarlet fever, diarrhœa, cholera infantum, pneumonia, and dysentery. For the year 1873 the deaths reported from these diseases, of those aged under five years, were for each disease in the order above, 361,

326, 302, 258, and 252. If the deaths from diarrhœa, cholera infantum, and dysentery be grouped together, and the deaths from "bowel diseases" be added, the aggregate number is indeed large. There is not much difficulty in fixing the actual cause of the excessive mortality from these specified causes among the conditions connected with the hot season of the year. It is reasonably certain that the mortality from these causes is greater in cities and villages than in rural districts. He has no hesitation "in subscribing to the belief that much of this infant mortality could be prevented by thorough and enlightened action by local boards of health in cities and villages."

Inflammation of the lungs is one of the prominent causes of infant mortality in Michigan. As to the actual cause of this disease, if the evidence already collected in the vital statistics of the State shall be verified by further examination, much of the infant mortality from this cause may be prevented by intelligent action of those who may control the condition of the air in dwellings and schools, as regards purity and humidity, but more especially as regards the warmth and humidity of the atmosphere in the sleeping rooms of the children. The greatest number of deaths from this disease seems to occur in a cold and dry atmosphere.

On the question of scarlet fever, Dr. Baker expresses his belief in the preventability of much of the mortality in that disease, because the disease is less fatal among persons who have passed the age of five or six years. Unfortunately, he has not yet such records of cases of the disease in this State as will enable him to verify or disprove this belief; but, if it be true, then prompt isolation of first cases, and thorough destruction of the contagium, will result in saving the lives of many by postponing the time when the disease shall be contracted. Whether the mortality is or is not less after the infantile period, many lives may be saved by the means suggested; for to some thus shielded in infancy the contagium may never again come in sufficient force to cause the disease. It seems probable that, even with no systematic effort for that purpose, many persons now pass through life without ever having the disease. It is the solemn duty of local boards of health to largely increase this number. He regards the great slaughter of the innocents in Michigan, by this disease, as a stinging reproach to the local boards of health for their inefficiency, and a powerful argument for the need of a State board of health charged with the noble duty of educating the people in the way of life.—[*Report from VII Public Health Report—American Public Health Association, 1876.*]

[Owing to pressure on space, our usual "Summary" is held over this time.—B. W. R.]

Letter of Reclamation from Dr. Meryon.

DR. MERYON writes to us to draw our attention to his writings upon "Muscular Atrophy and Paralysis," with reference to two Reviews lately published in this Journal upon "Bulbar Paralysis," and on "Muscular Atrophy" (Nos. cxv and cix), in which Dr. Meryon states that we have omitted to notice papers on this subject by himself read before the Royal Medical and Chirurgical Society,

and in which he claims to have been the first to describe the difference between Paralysis from Muscular Degeneration, and Muscular Atrophy from Centric Causes.

Dr. Meryon observes: "I do not explain, but when doubt is expressed, even at the present day, relative to the Myopathic Origin of Muscular Atrophy, it might have occurred to the author of the article on 'Bulbar Paralysis,' as well as to the author of that on 'Muscular Atrophy,' to remark on certain distinctive symptoms and pathological changes on which I insisted, as being characteristic of Myopathic Muscular Atrophy. The muscular trembling which is generally seen in Muscular Atrophy, dependent on Sclerosis of the Spinal Cord, I have never observed in pure Myopathic Muscular Atrophy. The pain which is generally observed in the one I have never observed in the other, and the pathological condition of the ultimate muscular fibre is unquestionably different in the two forms. In the myopathic muscular atrophy the sarcous granules are found dispersed and the sarcolemma broken down, whereas in atrophy arising from diseased nervous centres the fibre gradually dwindles—quickly so if it be a case of pseudo-muscular hypertrophy and oil globules are seen in abundance. In these latter cases the carmine dye betrays the central lesion, but I have failed to find any evidence of such lesion in the eight cases of pure muscular atrophy which I have described."

Our Reviewer observes that he is himself fully alive to the deficiencies of the articles referred to, and that he is made still more decidedly so by Dr. Meryon's note, inasmuch as the views held by that gentleman are confirmatory of the opinions expressed by the Reviewer, who furthermore desires to express his extreme regret that by a scarcely excusable oversight he has failed to avail himself of the valuable contributions of Dr. Meryon in the 'Medico-Chirurgical Transactions,' and in the work entitled 'Practical and Pathological Researches on the Various Forms of Paralysis,' published 1864.—(REVIEWER.)

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THE
BRITISH AND FOREIGN
MEDICO-CHIRURGICAL REVIEW.

APRIL, 1877.

Analytical and Critical Reviews.

I.—Diseases of the Respiratory Organs.¹

THE favorable opinion which we expressed in our number for April, 1875, of the then published parts of Ziemssen's 'Cyclopædia of the Practice of Medicine' is confirmed by a perusal of the articles on diseases of the respiratory organs contained in the fourth and fifth volumes of the English translation.

During the last twenty years the study of practical medicine has nowhere been carried on with more zeal and success than in Germany; of this the volumes before us afford ample proof, and to a consideration of their contents we now invite the attention of our readers, premising that, as the subject of pneumonia and phthisis have recently been specially and separately discussed, we shall in this article only superficially refer to them.

The contributions on croupous and catarrhal pneumonia, on hypostatic processes in the lungs, and on pneumonia from embolism, are by Professor Juergensen, of Tübingen, who, although comparatively young, has established a high reputation as a physician and teacher of medicine. They are translated by Drs. Ball and Frankel, of New York, and by Dr. Schaufler, of Kansas City, and the task has been well and faithfully done.

Rokitansky's division of inflammatory or pneumonic conditions of the lung into croupous and catarrhal is generally adopted in Germany, but has not been commonly accepted in this country. Indeed, our latest and best writer on pneumonia, Dr. Wilson Fox, discourages the use of these terms, as conveying a false impression—the exudation in sthenic pneumonia

¹ Ziemssen's *Cyclopædia of the Practice of Medicine*, English translation, vols. 4 and 5. 'Diseases of the Respiratory Organs.'

being very different from that in croup, and also from there being no boundary-line between the two forms of disease. Yet they are so essentially distinct in the character of the effusion,—corpuscular in the one case, fibrinous in the other, in their course, duration, fatality, and structural changes, that it seems desirable to characterise each by a specific name, and until better designations be introduced, we propose to employ the German nomenclature.

Thanks to the progress of pathological anatomy and to the more exact modes of exploration now in use, our knowledge of lung-disease has of late years become more complete and accurate. Much, however, remains to be accomplished. In the field of etiology we are specially in want of more reliable information. General records are only approximatively true, and hospital returns are insufficient as applying only to limited and variable conditions. In supplying these wants much may be done by the careful observations of physicians engaged in private practice.

The statistics which Professor Juergensen quotes in his chapter on the etiology of croupous pneumonia are diminished in value from the fact, that they obviously apply to both forms of the disease, and, in reality, chiefly relate to catarrhal pneumonia as being, by far, the more fatal.

Many interesting questions as to the causes of pneumonia still remain unsettled. Thus, it is doubtful if it be more prevalent in a cold climate, and in an elevated and exposed situation, than under opposite conditions. On this point two great authorities, Ziemssen and Hirsch, entertain contrary opinions. It is remarkable that, in Iceland, the mortality from pneumonia is only 13 per 1000 deaths, and that it is also very low in Munich, a city which stands high and is exposed on all sides.

Rapid changes seem to have more influence in its production than either an absolutely high or a low temperature. It is clear that it occurs in all latitudes and climates, and differs notably from brouchitis and catarrh, the prevalence of which increases as we advance from the tropics to colder latitudes. An outdoor life seems favorable to its escape,—thus sailors suffer but little from it; soldiers chiefly when in garrison, whilst it is common in prisons. Grisolles statement that employments which require much muscular exertion, which are carried on in the open air, and which expose workmen to inclemency of weather, count twice and a half as many victims to pneumonia as sedentary work exercised in close rooms and sheltered situations, is well criticised by Juergensen as being strangely in opposition to fact.

Croupous pneumonia seems to recur with tolerable uniformity

in annual cycles,—liable, however, to increase from time to time. Ziemssen believes that there is a remarkable coincidence between the prevalence of typhus and that of pneumonia, but doubt is thrown upon this by the Vienna and Stockholm statistics.

Passing over the chapters on the pathology of the disease, and the analysis of individual symptoms, we come to Juergensen's observations on crepitation, in his account of the physical signs presented in pneumonia. Laennec's statement that "the crepitant râle is pathognomonic of an inflammatory engorgement of the lung" is too dogmatic and exclusive, though it is perfectly true that the râle is rarely absent in this disease. According to Wintrich the only necessary conditions for the production of crepitation are, that the slightly swollen walls of the small bronchial tubes and air-cells should come in contact during expiration, and should be forcibly separated during inspiration. He maintains that the presence of a viscid mucus is not, as generally stated, indispensable for this result. Hence it is admitted that a crepitant râle may be heard (1) in the commencement of œdema of the lungs; (2) in the lower and back part of the lungs of patients with prolonged dorsal decubitus; (3) in some cases of pleuritic exudation, the lung being merely retracted, not compressed; (4) at the outset of acute catarrh of the small bronchi; (5) in the first and last stages of croupous pneumonia.

Professor Juergensen calls attention to a phenomenon of frequent occurrence in pneumonia, and a possible source of error. With extensive consolidation in the lower lobe of one lung, there may be heard on the fourth or fifth day sounds seeming to indicate commencing infiltration in the base of the opposite lung. Percussion may be slightly dull, with a tympanitic resonance, and a crepitant râle with sonorous and blowing sounds may be audible. They are simply due to retraction of the sound side,—diminution in the amount of contained air, with transmission of the râle and rhonchus from the diseased side. Bearing in mind the following points will prevent mistake: (1) The bronchial respiration becomes stronger the nearer we approach the seat of undoubted disease; (2) vesicular respiration may be heard through the blowing sound, if the patient be made to respire deeply; (3) the constitutional symptoms should be carefully estimated; in double pneumonia there are serious circulatory and general disturbances which are absent in these cases.

The truth and value of these observations we can confirm from our own experience. Accumulating evidence tends to prove the correctness of the statement that the most severe

cerebral symptoms usually occur in pneumonia of the apex,—setting in with high fever.

There is some discrepancy in writers as to the characters of the urinary secretion in pneumonia; thus Juergensen says the excretion of sulphuric acid is generally diminished; Dr. Wilson Fox that it appears to be “slightly increased.” So as to phosphoric acid: the latter writer states that it is lessened; Juergensen that it is variable, but sometimes much above the normal standard. Again, the diminution of chlorides in the urine has been adduced both as a diagnostic and prognostic sign. But J. Vogel has shown that the reduction in the amount of chlorides in the urine in febrile diseases is owing to the fact, that when the supply of chlorides is diminished, the body retains whatever chlorine is set free by the consumption of its tissues.

Our knowledge of the tissue or nutritive changes which take place in pneumonia is still very limited; the investigations of Reisell and Huppert on the transformation of nitrogen (Leipzig, 1869) being the only scientific and serious attempt in this direction with which we are acquainted. They found that both during the stage of fever and that of absorption of the exudation, the elimination greatly surpassed the ingestion of nitrogen. The excess was, of course, derived altogether from the body in the first case, and partially from the exudation in the second. Not until convalescence was fairly established did the two processes balance each other. These researches show, as Juergensen remarks, that the disturbances which occur in pneumonia are of a very profound character. As to the discharge of carbonic acid and the metamorphosis of inorganic matter, we know next to nothing.

The view formerly entertained, that pneumonia is a local disease, is now generally abandoned. As Dr. Wilson Fox observes, it depends upon an altered composition of, or some morbid matter in the blood. Probably many such conditions may have the power of inducing lung-inflammation. Professor Juergensen is a warm advocate of the doctrine that croupous pneumonia is a constitutional complaint. He refers it to the group of infectious diseases, and regards the pulmonary inflammation as merely the chief symptom. He founds his belief upon various facts bearing on its etiology, morbid anatomy, and pathology.

One of the strongest arguments in favour of the specific character of pneumonia is, that it cannot be produced by any of the usual causes of inflammation. As in typhoid fever, there must be a special exciting cause. The laws which govern the occurrence of pneumonia, and such diseases as bronchitis and

pleurisy, are altogether different in their relation to season and geographical distribution. The pathological arguments are also convincing. During the course of pneumonia there is no constant relation between the local and febrile symptoms; there may be high fever with a small amount of condensation, and slight fever with considerable exudation. Again, croupous pneumonia runs a typical course. "No affection which arises from a local lesion presents a career so definitely limited in point of time as is the case with croupous pneumonia."

These observations prepare us for Professor Juergensen's rational and philosophical mode of dealing with cases of pneumonia. He remarks that we have not to combat an inflammation but a constitutional disease, and one of comparatively short duration. Hence we naturally fall into an expectant system, interfering only when necessity requires. Seeing that of every 100 cases 64 terminate normally between the fifth and eighth days, we ask what is the value of plans of medication where a crisis occurs but little, if at all, sooner, and where recovery is probably slower?

In some rare cases danger may result from the amount of fever present, or from the extent of the pulmonary infiltration; but in most instances where risk exists, it is the conjoined influence of these upon the heart which occasions it. There follows in consequence this leading proposition: "the danger in croupous pneumonia threatens principally the heart of the patient." For proof and illustration of this we must refer to the work under notice. We fully agree with our author that the increased labour of the heart, produced by the exudation and consequent resistance to the pulmonary circulation, as well as by the lessened contact between the blood and air in the lungs, calls for the first for supporting and prophylactic measures. In carrying out these a careful study of the pulse is all-important.

In combating the fever Juergensen asks, Is it proper to use cold bathing in a case of pneumonia? He admits that there are objections to it, and that by contracting the peripheral vessels it increases for the time the heart's work. But he appeals to experience, which he affirms has established the utility of the direct abstraction of heat. He even recommends that where bathing is impracticable the body should be freely exposed to cold air until the necessary amount of cooling is obtained. But he insists strongly that a patient should never be bathed without the previous and subsequent administration of some stimulant; and that its amount should be increased if the water be absolutely cold. In addition, he always gives quinine, and in large (30 grains) dose, every second evening.

If the fever be high and the patient a strong adult, he carries the dose of quinine to 77 grains ; again justifying his proceeding by the results of his practice. He advocates a supporting regimen, and allows a liberal quantity of some light wine. As regards the very large doses of quinine, we cannot but think that a considerable portion of the drug must be lost ; we doubt also if English stomachs would tolerate them. But we fully agree with Professor Juergensen as to the great value of quinine in reducing temperature without impairing the power of the heart. Where a strong dose is needful we prefer to administer it by enema. It need hardly be stated that the use of depressing agents, such as tartar emetic or veratrine, is condemned.

An ample and carefully collected body of statistics is still wanted to testify to the full value of the modern treatment of pneumonia. Those hitherto published speak strongly in its favour. Thus, at Basle, where, however, the mortality is exceptionally high, the reduction from the rate obtaining under the old system, is nearly ten per cent.

Juergensen's historical notice of the recognition of *catarrhal pneumonia* is somewhat imperfect, but with this exception his essay is marked by the same fulness and clearness as his previous treatise. We shall not attempt, however, to follow him in detail, but simply notice one or two points of interest. Catarrhal, or lobular, or broncho-pneumonia, the disease of childhood and old age, has been only discriminated of late years. Dr. Gerhard, of Philadelphia, published in the 'American Journal of Medical Science' for 1834 a remarkable paper on the pneumonia of children, in which he clearly distinguished between the lobar or primary and the lobular or secondary forms of lung-inflammation. We believe that to him is due the credit of establishing the separation, which he based upon a careful analysis of his own observations, in conjunction with those of M. Ruz. He described the first as usually occurring in those previously in good health, and as being attended with but little risk ; whereas the second showed itself in the course of other diseases, and was very frequently fatal. A few years later MM. Rilliet and Barthez again called attention to the differences between the common pneumonia of childhood and that of adult age. They regarded, however, capillary bronchitis, lobular pneumonia, and lobar pneumonia, as varieties of the same malady ; there was but a step from one to the other ; one might be transformed into another ; and the only anatomical variation between them was one of extent. Since 1843, when their treatise on the diseases of children was given to the world, the subject has been carefully studied, and our ideas have gradually gained more clearness and consistency.

Except when produced by the inhalation of some irritant gas, as ammonia or chlorine, catarrhal pneumonia is always a secondary morbid process, and is preceded by inflammation of the bronchial mucous membrane. Hence its frequency after diseases associated with catarrh. It has no fixed type of fever; and not unfrequently runs a lengthened and irregular course. Of the anatomical changes induced by it pulmonary collapse is a common and important one, and granulation is never seen. It may occur with croupous pneumonia, and hence a source of perplexity which the deadhouse does not always remove.

As regards the physical diagnosis, which is sometimes a matter of difficulty, Professor Juergensen makes one or two remarks which deserve quoting. He recommends that percussion should be employed in zones around the chest, thinking that the smaller foci of condensation may thus be more readily detected. As Ziemssen observes also, in children allowance must be made for changes in the state of the lungs produced by their cries and importunities. Of auscultatory signs the most valuable is a crackling liquid sound, very similar to a crepitant râle, but less fine, and heard during expiration as well as inspiration. Over a collapsed portion of lung, the breath sounds will of course be very feeble; and where the bronchi are loaded with secretion, the sounds will be modified accordingly.

The rate of mortality in catarrhal pneumonia is very high, ranging from one third to one half of those attacked. Both Ziemssen and Juergensen agree that in acute attacks the danger is less than where the disease is slow and insidious. The younger the patient below puberty, the greater is the danger.

For our knowledge of hypostatic states of the lung we are mainly indebted to French observers, and especially to Piorry, who proved that they did not, as was supposed, take place during the death-struggle, or after death. Two conditions are needed for their production—diminution in the power of the heart, and prolonged maintenance of the body in one position. The circulation is retarded in the most dependent parts of the lungs, and the blood-vessels become incapable of retaining their contents. Juergensen agrees with Piorry that the condition is not one of inflammation, but may readily pass into it. In this way is occasioned that state of lung called *splenisation*, formerly regarded as a variety of pneumonia, and as due to inflammation of the intervesicular tissues. The proneness to the development of this condition in the old and cachectic must never be forgotten, nor the employment of due prophylactic means.

To the labours of Virchow, Panum and Cohnheim we are indebted for most of what we know respecting pneumonia from embolism and metastatic abscess. Professor Juergensen

gives a good summary of what has hitherto been made out, but without adding any fresh matter. The subjects of anæmia, hyperæmia, atrophy and hypertrophy of the lung, hæmorrhage, collapse, emphysema, gangrene, new formations and parasites, are treated very clearly by Professor Hertz, of Amsterdam.

Bleeding from the respiratory organs is considered under the three heads of bronchial hæmorrhage, hæmoptoic engorgement, and pulmonary apoplexy. Of these the first has been so carefully studied by Rasmussen and Rokitsky, and their researches are so well known that we need not dwell upon it. But our views as to the mode of production of pulmonary engorgement or infarction have undergone a complete change since the publication of Virchow's observations on embolism and thrombosis. Rokitsky at once accepted, and Cohnheim has since confirmed the theory that infarctions arise from occlusion of the small branches of the pulmonary artery. Generally associated with heart-disease, especially such as is accompanied by dilatation of the right side, they are also met with in cases of thrombosis of the peripheral veins of the body. Cohnheim's experiments have proved that when an embolus finds entrance into a branch of the pulmonary artery, infarction results if the vessel be a terminal one. But in some instances this explanation will not apply, and the rupture seems due to increased blood-pressure in small vessels, the walls of which have undergone degeneration.

The relations of broncho-pulmonary hæmorrhage with phthisis have recently been the subject of considerable discussion. F. Niemeyer, reviving the old doctrine of Morton and Sydenham, has taught that the effused but not expectorated blood in a patient suffering from hæmoptysis, lodging in the pulmonary cells, may give rise to inflammation, cheesy transformation, and disintegration. Hertz does not accept this teaching. He asserts, moreover, that blood extravasated in the pulmonary alveoli only occasions catarrhal pneumonia, and that this is never followed by caseous degeneration. The matter is still one for careful investigation, and we especially need trustworthy testimony as to the prior health of those in whom hæmoptysis seems to initiate active lung disease.

Since Laennec's day pulmonary emphysema has been carefully studied both in this country and on the Continent, and although nothing can surpass the accuracy of his description of the disease, we have advanced considerably upon his ideas of its pathology and mode of production.

Rokitsky first gave precision to our notions by presenting it as a combination of dilatation of the air-cells, with atrophy of the lung-tissue.

Hertz gives a full and fair account of the current theories as to the development of emphysema. The first is the old doctrine of Laennec, which ascribes it to deep and forcible inspiration, employed to overcome the resistance of accumulated bronchial secretion, or occasioned by a swollen condition of the mucous membrane of the small bronchial tubes. Some additional influence was attributed to expansion of the contained air, but this is a phenomenon of questionable occurrence. The second has been advocated by Dr. C. J. B. Williams, and is doubtless correct, but of limited application. It explains the origin of what has been called *vicarious* emphysema. A portion of lung being impenetrable to air, the inspiratory pressure tells with increased force upon, and produces dilatation of, the adjoining air-cells. But in many of these cases the influence of textural changes and increased expiratory pressure also come into play. The third and most important theory was first advanced in 1845 by Fuchs and Mendelssohn in Germany, and again, without knowledge of their writings, by Sir Wm. Jenner, in 1857. Fuchs showed that if forcible expiration took place, the glottis being narrowed or closed, the air within the chest being subjected to much pressure, distended the alveolar cells unless counter-pressure from without was afforded by the walls of the thorax, or by its contracted muscles. Mendelssohn somewhat expanded this view, which is essentially that of Sir Wm. Jenner.

In addition, Hertz calls attention to the influence of modified nutrition of the pulmonary parenchyma in favouring the production of emphysema. Thus, he explains the so-called hereditary tendency to the disease, or its greater prevalence in some families than in others; its occurrence after illness, especially pneumonia, and its increased frequency as age advances.

Hertz's description of the general features and relations of the disease is well given, but does not call for any special notice. We pass on to his observations on treatment by the inhalation of compressed air. For this a distinctly curative action has been claimed. Bertin says that of ninety-two cases thus treated, sixty-seven were perfectly and permanently cured. But if the cases were those of genuine emphysema, we confess to being incredulous as to the completeness or durability of the result. That considerable relief may be afforded by this measure is undeniable. It seems to have a calmative influence on both the circulatory and respiratory organs, it promotes the return of venous blood, diminishes the hyperæmia of the bronchial mucous membrane; whilst the increased introduction of oxygen and exhalation of carbonic acid improves the condition of the blood, and thus aids nutrition and tissue-metamorphosis. The act of expiration being difficult and imperfect, the attempt has been

made to facilitate it by sucking out, as it were, the air from the lungs, the patient being made to breathe into rarefied air. J. Hauke devised an apparatus for this purpose, which has been improved by Waldenburg, and now forms a simple and manageable instrument, which may be easily employed in private practice for the inhalation of either condensed or rarefied air. Under this impression we recommend it to the trial of those of our readers who are called upon to deal with this very distressing disease.

To Laennec's excellent powers of observation we are indebted for our first accurate description of pulmonary gangrene; but its discrimination from putrid bronchitis, and the fuller unfolding of its pathology, have only been effected of late years. Hertz gives an admirable summary of all that has hitherto been made out respecting it,—following very closely the lines laid down in Oppolzer's 'Vorlesungen.' The influence of emboli in its production, especially when proceeding from infectious purulent centres, is especially noticed. Such conditions are sometimes presented after amputations and parturition, in the course of disease of the bones and abscess of the liver, and in gangrenous bedsores. His description of the sputum in those cases of gangrene of the lung where a communication has been established with a large bronchus is very complete. Fischer's discovery in it of vibriones was confirmed by Leyden and Jaffé, who also found them in the bronchial tubes in a post-mortem examination. They also detected minute thallus-like threads, and rows of granules arranged in thready chains. They regard them as fungi, and name the growth *Leptothrix pulmonalis*. They found also that ordinary mucous sputum, allowed to putrefy in the open air, presented very similar characters to those of the expectoration in pulmonary gangrene. As pointed out by Traube, the needful septic conditions being air, moisture, and a certain temperature; these are found in the stagnating fluids of dilated bronchi, or dead portions of lung. "Here the germs of the *Leptothrix* seem to find a soil suitable to their development, and then give rise to further decompositions." The prognosis is not, as once supposed, absolutely unfavorable, as recovery occasionally takes place when the centre of disease is small and circumscribed. But in diffuse gangrene it is altogether hopeless. Hertz thinks that Skoda has created a new era in the therapeutics of the affection by introducing the treatment by inhalation of the vapours of various substances. Amongst these may be mentioned turpentine, chlorine water, carbolic acid, and bromine. If these remedies effect nothing else, they at all events diminish the

fetor of the breath and expectoration, and add materially both to the comfort of the patient and of those around him.

Of new growths in the mediastinum and lung, excluding the products of syphilis, leprosy, glanders, and lymphatic enlargements, Hertz enumerates the following: dermoid cysts, fibroma, lipoma, enchondroma, osteoma, sarcoma, myxoma, and carcinoma. Some of these are of small importance, being unrecognisable during life, and generally discovered by accident after death. This is not the case, however, as regards carcinoma, of which all the varieties are met with. It may be primary or secondary, and may occur in distinct masses, or in the form of medullary infiltration. To us, as physicians, the chief interest attaches to the diagnosis of the disease. This, in the diffused form, where cancerous nodules are disseminated through the lung, is often a matter of difficulty; and especially so when, as sometimes occurs, the case is complicated with pleuritic effusion. In general, the phenomena of compression are less marked in these cases than when the new growth presents itself as a distinct mass or tumour. Before arriving at a conclusion, it is essential that secondary and disturbing conditions be eliminated. It is well to bear in mind also that the dyscrasic or constitutional effects are sometimes wanting. As regards the face and complexion, they are apt to be obscured by the supervention of cyanosis and turgescence. Of the chapter on parasites of the lungs, animal and vegetable, it is sufficient to say that it contains an epitome of all the information which has hitherto been acquired respecting them.

The articles on pulmonary consumption and acute miliary tuberculosis are contributed by Professor Ruehle, the director of the medical clinic at Bonn. They are very carefully written and complete; but as the subjects of phthisis and tuberculosis have been recently separately considered, we shall not attempt an analysis of them, but simply quote one or two observations of interest. And, first, as to the change of opinion respecting the nature of pulmonary phthisis. For a long time it was regarded as tuberculous. But recently, experiment and the microscope have shown that what was formerly regarded as tubercle is partially a neoplasm, partially the product of inflammation. The latter again has been subdivided into several varieties, and the question rendered more complicated. Tubercle, strictly so called, had really no part, it was found, in tuberculosis; in fact, it might be produced in animals by any kind of inoculation. Its right to a separate existence was even called in question. "In opposition," says Professor Ruehle, "to this analytical tendency, there is beginning to be felt a synthetical necessity for simpler views." It is admitted, however, that Bayle's

miliary tubercle plays a subordinate part in pulmonary consumption, being an accidental secondary product. When it is the sole anatomical lesion, we have to deal with an acute infectious disease, miliary tuberculosis, which does not belong to phthisis. This "pernicious phthisis," as it has been termed, is a specific variety of inflammation with the characteristic caseous metamorphosis.

Professor Ruehle does not share F. Niemeyer's opinion as to the influence of broncho-pulmonary hæmorrhage in producing phthisis. He regards it rather as not unfrequently the first indication of disease, believing that antecedent lesions may exist, notwithstanding our inability to detect them. He remarks that foci of considerable size may escape notice, if distant from the surface of the lungs, and separated by air-containing tissue. He thinks that inflammation and caseous changes produced by the irritation resulting from decomposing blood in the bronchi are altogether exceptional. As regards miliary tuberculosis, he observes that the results of inoculation experiments have been so uniform, that we cannot avoid the conclusion that in man also it proceeds from infection, and generally from the patient himself. Of this we know of but one source, the absorption of caseous matter. Waldenburg supposes that a transportation of corpuscular elements takes place. Both lymphatics and blood-vessels are concerned in this result. It may originate in caseous lymphatic glands, due to measles or scrofula,—caries of bones; caseous affections of the testes, prostate, or bladder; inspissated abscesses; the remains of serous inflammations; and ulcerations in mucous membranes. The latter probably account for the acute miliary tuberculosis which sometimes occurs after typhoid fever. Other morbid states may also give rise to it; and one of the most frequent is pulmonary phthisis.

The volume concludes with a very important and original article by Professor Rindfleisch, of Wurtzburg, who, to use his own words, attempts once more to enlarge the domain of pulmonary tuberculosis.

He regards the reaction against Laennec's exclusive views as to the solely tuberculous nature of phthisis as having proceeded too far. He returns to the old doctrine of the intimate alliance of scrofula and tubercle, and affirms the general occurrence of tuberculosis in a special class of persons,—the scrofulous. He maintains the histological identity of scrofulous and tuberculous new growths, and thinks it not improbable that the tuberculous poison may proceed from the decomposing detritus of the matter effused in a scrofulous inflammation. Of this a characteristic is cellular infiltration of the connective tissue, which is thus converted into a hard, dense, greyish, half-trans-

lucent mass. In this the blood-vessels become occluded, and then necrotic processes ensue. This degeneration commences as cheesy transformation, first of the centre, then of the entire mass. To this, softening or calcification may follow. He thus considers tuberculous phthisis as a combination of scrofulous inflammation and tubercles, the latter being the result of the former. "There is first scrofula, and then a cachexia from the absorption of scrofulous products. The intensity of this is only partially revealed by the formation of miliary tubercles."

Professor Rindfleisch also expands the definition of tubercle. "Besides the cellular granulation-like tubercles, such as are met with in inoculated guinea-pigs, there is a fibroid tubercle, which I have found most frequently in syphilitic patients, and a lymphadenoid tubercle, which occurs as the rule in scrofulous patients." In the latter, the tubercle-cells are of unusual size; twice or three times as large as a white blood-globule; they are also highly refractive, with sharply defined and often shining nuclei.

Some observations upon the development of the disease deserve quoting. "The first lesion in pulmonary phthisis is a tuberculous infiltration of all the angles and projections situated at the points where the smallest bronchioles become continuous with the acini. In this way circumscribed white nodules are formed." The projections referred to are those of the circular edge of the bronchus, and the sharp, concave margins which separate the orifices of the three principal alveolar passages, and the infundibula. They contain fine muscular and elastic fibres, held together by connective tissue which is the seat of tuberculous deposit. "In this way is formed the primary tubercle of the lungs, Laennec's tubercle granulation."

"This constitutes the anatomical basis of the disease; a cellular infiltration of the pulmonary connective tissue, composed of small nodules, absolutely bloodless, not capable of suppuration, nor of resorption, nor of organization, but only of degeneration."

Our author believes also that in the catarrhal secretions of a scrofulous person is contained the tuberculous poison, which inoculates the edges of the narrowest portions of the bronchi. The projecting edges of the larynx, the vocal cords, and other parts of the respiratory mucous membrane may be infected in the same manner. He thinks also that the secretion if swallowed may produce tubercles in the lymphatics of the intestines. The tubercle granules are transformed after a time into the larger cheesy masses, which before Virchow's and Reinhardt's studies were called crude or yellow tubercle. The increase and extension of the disease seem to correspond to the course of the

bronchi. Although Rindfleisch does not object to Virchow's designation of this process as cheesy broncho-pneumonia, he prefers to qualify it as tubercular. He considers that there are three elements concerned in it, (1) tubercular ulceration of the bronchial mucous membrane; (2) tuberculous peribronchitis; (3) a scrofulous cheesy, or the so-called desquamative pneumonia.

Our limits will not permit us to follow Professor Rindfleisch in his description of these factors; we cannot, however, but add, that he considers himself to have proved that, of the peribronchitis, the essential part is the propagation of the disease through the peribronchial lymphatics. Of the dense, white tissue around the bronchi, he considers that on an average a third part is composed of conglomerated miliary tubercle; and that the remainder consists of a bloodless semitranslucent mass, the product of infiltration of the connective tissue. Lastly, desquamative or catarrhal pneumonia fuses these elements into the compact nodules, which are the acknowledged results of cheesy broncho-pneumonia; the degree and extent of this process varying greatly, however, in the different forms of pulmonary phthisis.

The high reputation which Professor Rindfleisch enjoys as a pathological histologist gives weight and interest to his views, for the fuller development of which we refer to the work under notice. It is clear that the whole subject is still open for further study.

The general diagnosis and therapeutics of diseases of the nose, pharynx, and larynx, as well as the subject of nasal diseases in general, are treated by Dr. Fraenkel, of the Augusta Hospital in Berlin. The notices of laryngoscopy, rhinoscopy, the instruments most useful in their practice, and the best modes of illumination, are especially full and clear, abounding with useful hints and recommendations. We shall be glad to see, in the medical schools of this country, an increase in the number of special teachers of these departments of practice, believing that our aids to their mastery are inferior to those which may be found in France and Germany.

The accomplished editor of the *Cyclopædia*, Professor von Ziemssen, of Munich, contributes a well-written article on catarrhal affections of the larynx. During the last eighteen years our knowledge of these diseases, and of the best mode of treating them, has been revolutionised. For this change we are largely indebted to the zeal and industry of our German brethren.

An admirable paper on Croup is the production of Dr. Steiner, of the Children's Hospital in Prague. Until lately, our ideas respecting this disease have been incomplete and inexact. It

is curious to find, on looking over its bibliography, that so long ago as 1784 Dr. S. Bard, of New York, recognised and stated that primary croup, membranous sore throat (doubtless diphtheria), and the same affection extending to the larynx, were three forms of one and the same complaint. His observations appear to have been forgotten when Bretonneau, in 1826, published his researches, and established the alliance of croup and diphtheria. But for many years subsequently, pseudo-croup or stridulous, and erythematous or ulcerative laryngitis, were confounded with the specific form of the disease. Hence we gladly echo Dr. R. H. Semple's call for a reform in the nomenclature of infantile laryngeal affections, as needful for accurate description and conception of them. For, in this country, we have been especially slow in admitting the diphtheritic character of croup; even in comparatively recent works on medicine we find it described as a simple inflammatory affection, the difference in the product of which (lymph instead of mucus) depending on the early age of its subjects, or on an excess of albumen in their blood, or on its being more deeply seated. Professor Steiner believes that the only difference between the lesion in croup and in diphtheria is, that in the former the exudation is *upon* the free surface of the mucous membrane, and in the latter it occurs also *within* it, thus giving rise to necrosis of the tissue. He regards the two affections as modifications of the same process; they occur together, shade into each other, present similar constitutional symptoms, and each may be followed by albuminuria.

True croup is very rarely seen a second time; out of more than 100,000 cases of disease in children, Professor Steiner has not met with a single example of its recurrence. Hence he characterises as fabulous all reports of its reappearance again and again. Such statements merely show that pseudo-croup has been mistaken for the true disease.

Several explanations have been offered of the extreme dyspnœa, which is so distressing in croup. The most obvious is the mechanical one, the glottis being narrowed by swelling and effusion. To this some suppose that a state of spasm is superadded. Niemeyer believes that the laryngeal muscles are paralysed, from being permeated by the products of inflammation. Others regard it as due to the irritating effects of the exudation on the peripheral nerves, and consequent want of co-ordination in the respiratory movements. Steiner considers it to be the combined result of several of these causes, the most important being the swelling and congestion of the laryngeal mucous membrane and the accumulation of false membrane and muco-purulent secretion. Niemeyer's explana-

tion of the almost invariable concomitance of bronchial catarrh is accepted as partially true. When the thorax expands, without admitting a proportionate quantity of air, that already present in the bronchi and air-cells becomes rarefied, and acts upon the mucous membrane as an exhausted cupping-glass does upon the skin, producing hyperæmia and increased transudation from the reduced pressure upon the blood-vessels. It is objected that too little stress is laid upon the inflammatory element, and too much upon the physical cause of the catarrh.

As regards the pathological anatomy of croup, Professor Steiner says that in his examination of more than one hundred fatal cases, he has always found false membrane in larger or smaller quantity in the larynx. Of course, it is possible that in some necropsies none may be found, in consequence of its separation and expectoration before death. In the great majority of cases the mucous membrane of the pharynx is found to be involved; in a somewhat smaller proportion, that of the trachea and bronchi.

The terrible fatality of true croup is illustrated by our author's statement, that out of a very large number of cases occurring in his practice, before he adopted the operation of tracheotomy, he saw only three recoveries; since its employment, the mortality has ranged from sixty to seventy per cent. Hence he is a warm advocate of this proceeding, and advises its performance at once if symptoms of carbonic-acid poisoning occur.

The subject of tracheal and bronchial disease is treated by Dr. Riegel, who was for some years assistant to Von Bamberger at Wurtzburg, and is now the medical director of the great city hospital in Cologne. An ardent cultivator of medical science, and the introducer of stethography, he has handled his topics with great and almost redundant fulness. His chapter on malformation of the air tubes is interesting anatomically rather than clinically. Under the term bronchial catarrh he discusses the various forms of inflammation of the tracheal and bronchial mucous membrane, in which the secretion is first characterised by its richness in mucine, and at a later stage by the presence, in greater or smaller quantity, of cellular elements, especially lymph-corpuscles. An interesting section is devoted to the etiology of the disease, respecting which exact statistics are still wanting,—professional being but little in advance of popular information. Hirsch, who has paid special attention to the question, believes that humidity exercises a more important influence than cold in the production of this class of complaints; but its effect is certainly greatest when combined with a low and variable temperature. As regards what is really implied in "taking cold," it is thought that light may

be thrown by the experiments of Rosenthal, of Erlangen. He found that on removing animals which had been for some time in a hot enclosure, their temperature fell not only to the normal degree, but below it. He supposes that this is due to the weakened condition of the dilated vessels of the skin. If the heated body be *suddenly* exposed to cold, the blood in the superficial vessels is chilled, and traversing the internal organs affects them injuriously, especially if they are enfeebled from any cause.

From a microscopic examination of the secretion it has been proposed to distinguish bronchitis as mucous, purulent, and epithelial. The two former divisions merely express the ordinary evolution of the complaint; as to the last, Riegel agrees with Biermer, that ciliary cells are either altogether wanting, or present in very small number. Pavement epithelium from the mouth and vocal cords is met with abundantly; cylindrical epithelium occurs less frequently.

According to the stage of the disease, mucus and pus or white-blood-corpuscles present themselves in smaller or larger numbers; crystals of sebatic and margaric acids, and of ammonio-magnesian phosphate are often seen; plates of cholesterine and fungus growths more rarely. Riegel's account of the different forms of bronchitis is very complete, and his analysis of the most important individual symptoms is characterised by a closeness and minuteness which involves considerable repetition.

Attention is drawn to the observation of Ziemssen as to the great importance of noting the fluctuations of the temperature in the capillary bronchitis of children. In this disease, if uncomplicated, a heat higher than 102.2° F. is not common. A sudden rise to 104° F. and upwards generally indicates the onset of catarrhal pneumonia.

The chapter on prophylaxis and treatment is not less comprehensive than the preceding section of the article. The use of atomized fluids, vapours, and gases by inhalation is mentioned approvingly in both acute and chronic bronchial catarrh, although it is admitted that the value of the practice has been overestimated. The new remedy, apomorphine, is named as a good expectorant in the dose of one sixth to one half of a grain. As an emetic, the muriate of apomorphia is said to be unfailing, administered hypodermically in the dose of one thirteenth to one sixth of a grain. Antiphlogistics are only mentioned to be condemned. If venæsection be ever admissible, it is when accumulation of blood has taken place in the lungs and right heart, as indicated by intense dyspnœa, cyanosis, and dilatation of the jugular veins. Here, by

diminishing the heart's work, it may give temporary relief, but it is at the risk of increasing the subsequent danger. In the vast majority of cases the use of stimulants is safer. The testimony as to the good results from the employment of condensed and rarefied air in chronic bronchial catarrh is so strong that we shall be glad to see its general adoption in this country. The pneumatic cabinet is no longer needful. Hauke's apparatus improved by Waldenburg is portable and may be used without any difficulty.

The article on plastic or croupous bronchitis contains all that is known respecting this rare and obscure disease; and that on tracheal and bronchial stenoses and foreign bodies in the air-passages is not less exhaustive in its treatment.

The existence of pure asthma, or paroxysmal dyspnoea, independent of organic disease, a true neurosis, has been often denied, but is now generally admitted. Various theories have been offered of its pathology; in this country, and also in France, the doctrine of bronchial spasm has been all but universally accepted. In Germany opinion is more divided. To the oldest and most popular explanation, that of spasm of the bronchial muscles, several exceptions must be made. Wintrich denies that asthma can be the result of spasm of the tubes, basing his views mainly on the physical signs observed during a paroxysm. He thinks it impossible to harmonise the presence of enlargement of the lungs with contraction of the tubes, and considers that the reverse state should exist. He believes that the impediment to respiration is a tonic spasm of the diaphragm, or of this and the respiratory muscles together. Bamberger has given the weight of his name in support of this theory. He believes that the phenomena are best explained by a tonic spasm of the diaphragm, to which may be superadded in different cases, spasm of the inspiratory or the expiratory muscles. Biermer is strongly opposed to these views, and attributes the chief influence to the bronchial muscles. He thinks that the prolonged and laborious expiration which characterises an asthmatic seizure, necessitates the idea of bronchial constriction from spasm. He recognises the depressed position of the diaphragm as a constant occurrence in bronchial asthma, but ascribes it to the dilatation of the lungs. He not only regards as improbable the continuance of a diaphragmatic spasm for hours, but maintains that he has satisfied himself of the rhythmic action of the muscle during an asthmatic attack. He considers that the bronchial muscles are the antagonists of the muscles of inspiration, and aid in expiration concurrently with the elasticity of the lung. The inspiratory power is sufficient to drive the air through the constricted bronchi and

distend the lung, but its expulsion is slow and imperfect, although all the available expiratory power is summoned to effect it.

Another authority on the subject, Lebert, holds medium ground. His opinion is that the action is reflex, affecting the bronchial muscles first, then the muscles of the throat and chest, and lastly that the diaphragm is excited to a degree of tonic action, in which the abdominal muscles sometimes participate.

To these theories must be added two others which have been recently broached. Leyden, on examining microscopically the sputum from an asthmatic patient, found clusters of brownish (? epithelial) cells undergoing granular degeneration, between which were layers of crystals. They were pointed octohedra, of variable size, the nature of which has not been determined ; but which Leyden supposes induce by reflex action bronchial spasm, from their irritating effects on the peripheral extremities of the pneumogastric nerve.

Lastly, Weber believes that the essential condition is one of tumefaction of the bronchial mucous membrane, from dilatation of blood-vessels through vaso-motor nervous influence. Riegel is of opinion that one explanation will not apply to all cases. In some mild forms bronchial hyperæmia may be the sole condition. But in the severe types of the disease there must, he thinks, be a spasmodic element in addition. He sees proof of this in the sudden declension of an attack under the influence of morphia or chloral hydrate. Diaphragmatic spasm may sometimes occur, but he disbelieves entirely in its existence as an essential or primary phenomenon.

We note these discrepant opinions, which have not attracted much comment in this country, in order to ask for more evidence on the question.

The articles on pleurisy, hydrothorax, and pneumothorax are written by Dr. Oscar Fraentzel, one of the physicians to the large Charité Hospital in Berlin. They are very able productions—methodical and clear, full without diffuseness, and contain the most recent and best established doctrines. A few points chiefly bearing upon the physical diagnosis and treatment of pleurisy are all that our space will allow us to touch upon.

For the sake of accuracy, Fraentzel divides his notice of pleurisy into three stages, and describes the characteristics of each separately. We pass over what he says respecting the first, to dwell upon some interesting features presented by the second, which have been less studied in this country than in Germany.

At this period there is liquid effusion, but not such as to produce displacement of organs or expansion of the chest. Over its situation, and in proportion to its amount, the percussion-note is high and dull. If not prevented by adhesions, as the exudation increases, the lung is compressed, and recedes into the upper and back part of the chest.

Under these conditions the stroke-sound under the clavicle of the affected side is loud, deep, and tympanitic. The rationale of this Fraentzel bases upon the experiments of Wintrich and the clinical experience of Traube. The pitch of the note, he says, depends both upon the amount of the vibrating column of air and the tension of the lung-tissue. The greater the tension the higher the note, and the larger the volume of air the deeper the note. Now, as the effusion compresses the lung, the quantity of contained air is reduced, and at the same time the tension of the lung is diminished. These may balance each other, and the sound remain unaltered, but this is rarely the case. The decrease in tension is usually (proportionally) greater than the decrease in volume; hence the abnormally deep, tympanitic sound on percussion.

In the third stage, when the effused fluid has led to displacement of various organs and dilatation of the thorax, the stroke-sound is dull wherever the liquid has a depth of from an inch and a half to two inches between the lung and the walls of the chest. But above this limit the percussion-note is now high and dull as well as tympanitic. The retraction and compression of the lung are carried to their utmost degree, the volume of the vibrating column of air is greatly lessened, as is also the tension of the lung; hence the high note is the result of the over-balancing influence of the former over the latter.

This explanation is not altogether satisfactory; the loudness or intensity of the sound may be influenced by the greater or smaller amount of the oscillating air, but we have yet to be convinced that its pitch will be so modified. We think also that more should be attributed to the tracheal or bronchial note transmitted through the condensed lung.

Traube has usefully applied to practice the well-known fact that in the lower and anterior part of the left side of the chest there is a half-moon-shaped space where the stroke-sound is normally tympanitic from the resonance imparted by the neighbouring stomach and large intestine. Its presence or absence, or variations in its extent, afford valuable indications in left pleuritic effusion. The greater the amount of liquid, the more is the diaphragm depressed, and the smaller the semilunar space of which we speak. In extreme cases it disappears altogether, and there may even be an area of dulness beyond the margin

of the chest owing to the displaced diaphragm. Fraentzel complains that the diagnostic importance of these considerations is insufficiently estimated.

As regards auscultatory signs, our author agrees with Skoda in his estimate of *ægophony*. He regards it as an unimportant variety of *bronchophony*, and by no means characteristic of pleuritic effusion.

The subject of treatment is very fully handled, and a large space is devoted to a consideration of the lately revived operation of puncturing the chest. This proceeding, which dates from the time of Hippocrates, was long ago strongly recommended by Trousseau, in France, but has only of late years been accepted in Germany. Fraentzel is a warm advocate of its employment where the effusion is very large, or its absorption long delayed. He gives minute directions for the best mode of practising it, and a description of the precautions to be used, so that not a single bubble of air or infective germ can find entrance into the pleural sac. His experience of its usefulness has been both large and favorable, and he thinks that where adverse testimony has been borne respecting it, there have been faults in the time and mode of its performance. But we cannot regard the question as settled when we find that such careful physicians as MM. Behier and Peter, of Paris, state that during the last six years the mortality from pleurisy in the hospitals of that city has doubled, and are inclined to refer the increase to the free use of this mode of treatment. When the effusion is distinctly purulent, as proved by exploratory puncture, Fraentzel has recourse to the radical operation as he styles it, or that by incision, followed by irrigation of the pleural sac. It is a formidable measure, but its use is justified by the results which have been obtained. Of eleven cases operated upon at the time of his writing, complete recovery ensued in five, and a favorable issue was hoped for in a sixth, then under treatment.

We here close our notice of these excellent volumes, the study of which may be confidently recommended. A few verbal errors and local peculiarities of expression have crept into some of the translations; they will doubtless be rectified in a second edition. The publication is in every respect creditable to the firm which has undertaken it.

II.—The Diffusion of Cholera.¹

THOUGH much information has been collected of late years regarding the manifestations of cholera in different parts of the eastern and western hemispheres, in its various diffusions, yet from many extensive tracts there is scarcely more than a meagre notice, without specification as to the time of appearance or cessation, the space occupied, or the connection with outbreaks elsewhere. The value of more detailed evidence on these points will therefore be readily appreciated by every one who has examined the subject, and every addition to the existing information, whether from a country the epidemics of which have not been fully described, or from one where their appearance has not hitherto been recorded, will be regarded as an important contribution to our knowledge, and as adding to the stock of materials which may ultimately enable us to arrive at a trustworthy explanation of the rapid and extensive migrations of this disease.

To promote this object, in 1870 Mr. Netton Radcliffe, then honorary secretary to the Epidemiological Society of London, through Bishop Tozer, invited the co-operation of Dr. Christie, physician to the Sultan of Zanzibar, in collecting facts regarding the epidemic in that locality, which was just disappearing. This gentleman, in response to Mr. Radcliffe's application, transmitted two papers, which were read at the Epidemiological Society in January, and February, 1871; and a favorable notice of these in the 'Lancet' induced him, subsequently, to investigate the tracks of the various epidemics mentioned in them, for which his position at Zanzibar, and inquiries as to the trade routes in Eastern Africa he was engaged on at the time, offered unusual opportunities. "It was not till then," he says in his preface, "that I had any conception that remarks regarding the track of a cholera epidemic, in a country such as East Africa, could be possessed of any possible interest."

The works of Livingstone, and other travellers in East Africa, will have familiarised some of our readers with the course of trade in that part of the world; for the information of others, we may mention that Zanzibar is the centre of mercantile transactions on that coast, the point to which the imports from other countries converge, and from which they are distributed to the

¹ *Cholera Epidemics in East Africa.* By JAMES CHRISTIE, A.M., M.D. London, 1876. Svo, p. 508.

Report by Mr. NETTEN RADCLIFFE on the Diffusion of Cholera, and its Prevalence in Europe, during the Ten Years 1865-74. In Report of the Medical Officer of the Privy Council and Local Government Board. New Series, No. V. London, 1875.

various towns on the mainland, north and south of it, from Cape Guardafui to Cape Delgado, where they are either sold to the native consumers in the vicinity, or are sent by caravans to variable distances into the interior, and then disposed of to those tribes who do not visit the coast. Dr. Christie informs us that

“There are generally three leading men who have a pecuniary interest in the caravan—an Arab of high standing in Zanzibar; an influential coast-man, such as the native Governor of the coast town; and one or more Banyan merchants. One or more Arabs often accompany the caravan, but the real leaders thereof are usually Wasawahili, or Wazalia—domestic slaves born in the family. They are always men who have been long in the trade, and who are thoroughly acquainted with the general features of the country; the people thereof; their language, and their manners and customs. One of the leaders of the caravan from Pangani to Laikepya had made fifteen journeys among the Masai, the other nine” (p. 222).

These men possess great shrewdness and powers of observation on all points coming within their immediate cognizance, or bearing on their interests, and most of the available information regarding the tribes occupying districts which have not been visited by Europeans has been obtained through their agency. By comparing the statements of several of these, at Zanzibar, Dr. Christie found a substantial agreement among them on the points he was investigating; and by engaging his friends at other points of the coast to prosecute similar inquiries there, he ultimately obtained an amount of evidence as to the appearance of cholera in different localities, which seems fairly reliable, and is of much interest. Besides these sources of information his position at Zanzibar brought him in contact, not only with several Europeans who had travelled in the interior, but, through the native merchants resident there, with traders from every point of the coast, who were often able to mention circumstances that otherwise might have escaped notice altogether.

Dr. Christie describes among ‘Epidemics of Cholera in Eastern Africa’ those of 1836-7, of 1858-9, of 1865, and of 1869-70.

Allusion is made to an epidemic in 1821, soon after Mauritius and Bourbon had been affected, but no details of it are now available; and there seems to have been another in 1852,¹ which

¹ In a parliamentary paper, dated Board of Trade, 20th August, 1860, entitled “Abstracts of Returns of Information on the Laws of Quarantine which have been obtained by the Board of Trade,” and which by a note appear to have been compiled by the Quarantine Committee of the National Association for the Promotion of Social Science, at page 78, it is stated, “Zanzibar and other places on the east coast of Africa suffered first from cholera, and afterwards from smallpox in 1852.”

has escaped Dr. Christie's notice. The first of the four epidemics mentioned above is stated by Captain Burton to have occurred at the end of 1835 and commencement of 1836, and Dr. Christie gave the same years in his first paper communicated to the Epidemiological Society, but he altered this in the subsequent one to 1836-37, in conformity with the date of a letter sent from Zanzibar to Captain Mohammed bin Hamees, who was then resident in London, intimating the death of his grandfather from cholera. The details of this epidemic are but scanty; it was first heard of in the Somali ports (Worsheik, Mukdeesha, Merka, and Brava), and it is said to have been carried along the coast during the north-east monsoon, by the native trading vessels, to Zanzibar, and from that onwards to the boundary of the Mozambique territories, where it ceased. When Somalis from Merka and Brava were questioned by Dr. Christie as to whether the cholera ever originated in their country, they maintained that in every case it had been conveyed to them from without. Further on he states,—“It was impossible to ascertain whether the epidemic of 1836-37 reached the Somali ports by sea or by land. I met with no one who at the time of invasion had reached the years of manhood, and all statements on the subject were evidently conjectures” (p. 103). Though we may accept the evidence of ordinary observers as to the dates of appearance and cessation of cholera, in various localities, so far as they may have come under their cognizance, we fear that many of the inferences they introduce into their recital of these facts are fairly open to the criticism which Dr. Christie applies to his Somali informants; and, in dealing with such statements, we must always be careful to separate simple matters of fact from the conjectures which the human mind seems so prone to mix up with them.

Dr. Christie is inclined to the opinion that this epidemic was an offshoot of that experienced at Mecca early in 1835, and that it might have been conveyed to Berbera by returning pilgrims, and thence, southwards, by the regular channels of communication through the Somali country, or by sea by trading vessels after the commencement of the north-east monsoon in November. “This,” he says, “would have exactly fitted the date of the epidemic originally given me, and that also described by Captain Burton; but I am unable to resist the evidence that the epidemic really occurred in 1836-37.” To meet the latter date he says, “It need scarcely be stated that cholera may have lingered about Southern Arabia and the Persian Gulf from the close of 1835 till the end of 1836,” and then have been carried southward in the manner indicated above. In submitting these views it appears to us Dr. Christie has inadvertently

fallen into the same error he remarked on in the case of the Somali; and, when we mention that there was a severe epidemic of cholera in the low districts of South Abyssinia in 1834,¹ he will find still another difficulty to surmount.

The next epidemic of cholera dealt with by Dr. Christie is that of 1858-59. In the autumn of the former year cholera had prevailed at the Arabian ports of the Red Sea, Gulf of Aden, and Persian Gulf, having trading relations with Zanzibar. With the disease present at these points at the very opening of the north-east monsoon, when the great annual stream of commerce was setting in, it was scarcely possible, he says, for Eastern and Central Africa to escape. It is usual for native craft from the Arabian ports to call, not only at the chief places (named above) on their way to Zanzibar, but also at several intermediate ports of less importance on the mainland and islands along the coast, to procure cargo and passengers. Lamoo and Mombassa are commonly visited in this way, and even by dhows from Bombay and Cutch. The epidemic seems to have broken out early in November, and the first intelligence of it received at Zanzibar was that it had appeared at Merka; it was next noticed as being at Lamoo; and the first case which reached Zanzibar was in a dhow that came from Brava and Lamoo at the end of November. Subsequently the disease extended southwards along the coast as far at least as Kilwa, and a very graphic description of its ravages there is given by Captain Burton, who visited that place in February, 1859, when it was at its height. The epidemic is said to have extended from the coast inland three or four days' journey only, but Captain Burton states that at Kilwa, although there were no precautions of quarantine, the contagion did not spread to the interior. The mortality along the coast seems to have been very great; at Zanzibar it continued from the end of November till the beginning of March, and it was estimated that 20,000 died in the island, of whom from 7000 to 8000 were in the town and suburbs.

Dr. Christie alludes to the conversation Mtesa, chief of Uganda (west of Lake Victoria Nyanza) had with Captain Speke, in April, 1862, regarding cholera, and is under the impression that he must have had some very recent occurrence in his mind, and expresses a strong conviction that this must have been the epidemic of 1858-59, which "had spread to Uganda direct from the coast to the north of Victoria Nyanza, by the old caravan route" (p. 116). This may be so, but it is in opposition to the evidence that it had extended inland only three or four days'

¹ Kirk, 'Transactions of Medical and Physical Society of Bombay,' No. vi, (1843), p. 24.

journey on the one hand, and, on the other, at variance with Mtesa's statement that "cholera had vanished as mysteriously as it came," and with his inquiry as to "What brought it?" for, had it arrived in his country with a caravan, he could not have failed to learn that fact, and would not have been slow in theorising upon it.

The third epidemic noticed by our author was that of 1865, which was first heard of in April, in the Somali ports. Mr. R. Brenner, who accompanied Baron von Decken in his expedition to the river Jub that year, states that they encountered the disease in the coast towns, while on their way from Zanzibar; and, while in the river, he made inquiries as to its line of approach, and was informed it had descended the river from Gannah to Barderah, and that it had reached Gannah from Berbera on the Gulf of Aden. From Barderah it passed to the coast at Brava by the trade route, and from thence northward to Merka and Mukdeesha, and southward to Patta, Lamoo, Melinde, and there were a few cases at Mombassa, but the disease did not advance further south, which Dr. Christie attributes to the south-east monsoon having set in early and strong, and shut off all traffic to the southward. At Melinde the epidemic passed on to the interior, and prevailed to a considerable extent in Ukambani and further inland. At Patta, and Lamoo, where the greatest mortality took place, it continued till June; farther north it seems to have extended to August, and not to have spread to the interior to any great extent. According to Mr. Brenner's statement the epidemic had ceased in the districts along the Jub, when the expedition entered it in June.

The great fair at Berbera, lasting from November till March, and attended by traders from the Red Sea and south coast of Arabia, as well as the Persian Gulf and India, is regarded by Dr. Christie as a point where there is great danger of cholera being introduced, and from which he thinks it could readily be diffused over the Somali districts, to the southward and westward, by the returning caravans. The account given by Mr. Brenner points to Berbera as the probable locality where the disease was first met with, but there is no record of cholera having been there in the end of 1864, or early in 1865. Dr. Christie says (p. 140), "I am not certain that cholera prevailed as an epidemic during the time of the fair," and subsequently (at p. 143), "account for it as we may, cholera must have been at Berbera, on the African coast of the Gulf of Aden, not later than December, 1864." It would have been more satisfactory to have had specific evidence on this question, but in its absence we think it would have been but prudent to admit

that a link was wanting in the chain of evidence at that point, which for the present, at least, he was unable to supply.

The last epidemic described by Dr. Christie is that of 1869-70, of which, thanks to his great desire to procure full information, and assiduity in following up every channel by which it might be obtained, we have more minute and specific details than for any of those which preceded it. About the beginning of May, 1869, a caravan from Pangani was trading at a town called Kisima in the province of Laikepya, just south of the equator, and about 37° east longitude. Here they were informed that the Masai had made a raid upon a tribe, to the north of Laikepya, called the Soma Gurra, and that they had brought back with them some deadly plague of which many of the Masai had died; the same disease was also prevalent among the people of Laikepya, attended with a great mortality, and one of the men of the caravan died suddenly here, but no illness occurred among the others. Alarmed by the rumours of sickness the caravan left the district, before their trading operations were completed, to return to the coast, and in August reached a town named Lytoketoke, north of Mount Kilima-njaro. During their journey from Kisima to this place rumours reached them concerning the mortality among the natives, but they had never met with a case of the disease; here, however, the leader of the caravan having seen some persons labouring under it, perceived at once it was cholera; the caravan forthwith returned to Pangani, and brought the first news of the approach of the epidemic. The disease itself was first seen in Pangani towards the end of October, and within a few days cases occurred in the island of Zanzibar, though its presence was not officially notified until 22nd November. During this interval there was constant communication by dhows with Pangani, where cholera was now prevalent; with reference to the apparent delay in the development of the epidemic in the town of Zanzibar, our author states:—

“If the native reports are to be believed, and I see no reason why they should be discarded, the earliest cases did not become epidemic centres, and many deaths occurred before the disease showed any tendency to spread. Single cases of cholera may thus be imported into a large community without a subsequent spread of the disease, the conditions for its spread being absent, but when a single case finds in a locality its appropriate material for propagation, it may appear with invincible violence, and poison the life fountains of an entire community. The epidemic of 1869 found its appropriate nest in November of that year in the household of a Hadramaut Arab in the district of Melinde” (p. 366).

It would occupy too much of our space to go fairly into the circumstances Dr. Christie alludes to in the last sentence, and we must refer our readers to the work itself for a detail of them.

This epidemic seems to have occupied the whole country from 1° N. to 6° S., between the meridians of 34° and 39° E., in the course of the summer and autumn months of 1869; the districts lying between the coast and the mountain range running north from Kilima-ujaro were affected later, after Pangani had been attacked. Mombassa and Melinde were affected in December, and Lamoo in January, 1870. Cholera is said to have reappeared in Lamoo in April, on the arrival of dhows from Zanzibar, and continued severely for four months. Brava, Merka, and Mukdeesha were also affected about this period, and continued to suffer severely for about two months. The country between the coast and mountains, south of the Jub, seems all to have been under the epidemic at this time; from statements by natives from the districts south of the Jub Dr. Christie thinks it probable a branch of the epidemic proceeded from the interior as far as Port Durnford on the coast some months earlier, and at p. 469 it is mentioned that Kismayo, at the mouth of the Jub, was infected in March, 1869; if this be correct it is to be observed that although the disease at that time showed no disposition to extend along the coast, either north or south, it was the same month that Dr. Christie has inserted in his map as the probable commencement of the epidemic among the Soma Gurra in the interior, and that Kismayo is at the nearest point on the coast of the country of the Soma Gurra, a relation which as we shall see immediately does not seem to have struck him.

The epidemic extended along the coast to the southward as far as Cape Delgado (11° S.), where it stopped. Dr. Christie, in the map accompanying the work, indicates a branch of the epidemic as having proceeded in 1869 from the south-east corner of the Victoria Nyanza to the southernmost point of the Taganyika, and from thence westwards to Lake Moero, and in the early months of 1870 the disease was epidemic at many points between this line and the coast, and, later that year, seems to have extended into the Manyema country, west of Lake Taganyika, where Livingstone then was.

The coast line from Cape Delgado to Mozambique remained unaffected, when, on the 6th May, 1870, a month after the commencement of the south-west monsoon, which stopped communication by sea from the north, the first case occurred there. Dr. Christie states that a branch of the epidemic proceeded overland from the vicinity of Lake Nyassa, but says: "The precise line of the epidemic from the Nyassa region to

Mozambique was not positively ascertained, but that it lay in that direction is certain. The trade movements in that district would be, at the commencement of the south-west monsoon, from the interior towards the coast" (p. 439). The disease continued at Mozambique, and in its vicinity, until March, 1871. In August and September, 1870, it extended to Johanna and Nossibey, two of the Comoro Islands, and to Majunga in Madagascar; and in January, 1871, Ibo, a small island on the coast, north of Mozambique, was affected, and Quillimane, to the south, the following month; and even in Mauritius, in December, 1870, and January, 1871, cases of severe sporadic cholera were not uncommon, and many of them fatal. The disease extended inland from Ibo about five days' journey, and from Quillimane along the course of the Zambezi; at the latter point it was still active in June.

The greatest mortality in Zanzibar was during the latter part of November and in December, the latter coinciding with the month of Ramazan and its fasts, which commenced on the 6th December; towards the end of January both town and island were nearly free from the disease. In March there was a large accession of strangers, in trading vessels, in the harbour and on shore, and the mortality among these in March and part of April was about fifty daily. In the latter half of April most of those bound northwards departed, but with the commencement of the south-west monsoon slaves from the southward, where cholera had been prevailing for some time, were brought in large numbers, and these continued to suffer so that in the harbour the disease was present during June; in the town at the same time there were merely isolated cases which continued until the middle of July.

Dr. Christie, who is a staunch supporter of the theory that cholera epidemics proceed from India, and are only propagated along the highways of human intercourse, believes that the track of that which has just been sketched was a continuation of the epidemic which, in 1865, affected the low districts north of Abyssinia, and, in 1866, attacked the higher ground of that country, and was at the south of Lake Tsana in May, and was last heard of at Devra Tabor and Gaffat in July. The district occupied by the Soma Gurra, where the epidemic was met with in 1869, is 2° north of the equator, and 38° east longitude, only some 700 English miles south from those places; and he tells us that, "for three years, 1867-69, nothing whatever was known regarding it beyond the unknown regions in which it prevailed" (p. 501). Upon this we need only remark that the history of cholera affords no instance of that disease continuing in activity for so long a period in so limited a space,

which did not contain one or more of its recognised endemic localities, or was not subjected to a fresh invasion. There is nothing to show the district in question includes any of these localities, neither was there any fresh invasion of Southern Arabia or the opposite coast of the Red Sea in 1867 or 1868. The appearance of the disease at Kismayo, too, in March, 1869, has not been taken into account by Dr. Christie in his explanation of the supposed course of the epidemic from Abyssinia, though it is a very important fact, and indicates a much greater extension of the disease early in 1869 than his theory recognises. Taking all the circumstances into consideration, we conceive there is no alternative but to admit that the theory of the transmission of the epidemic from Abyssinia by human intercourse has not been established.

The volume contains a large amount of matter concerning the distribution of the different tribes throughout the country, their manners and customs, and extensive extracts from the works of various travellers on these subjects. No doubt a summary of such information was required to enable readers to appreciate the various influences connected with the manifestations of the epidemics submitted for their consideration, but it appears to us this might have been given in a much more concise form, and thus have saved the time and obviated the trial of temper to those who are anxious to get at the material points of the history. There are some discrepancies, too, which may lead to doubt; thus, while we are led to suppose that uncontaminated water supply was intimately associated with immunity from cholera, and it is stated at p. 355 that that employed by the Europeans is purer than the best obtained by the Arabs or Banyans, we find at p. 291 it is said the Europeans are at the mercy of the female water-carriers, and obtain a mixture of inferior quality, "and only Arabs can manage to get a purer element." Again, it is mentioned, unconditionally, that the north-east monsoon, by stopping the traffic northwards, prevents the extension of cholera by sea in that direction, and the south-east monsoon, for a similar reason, stops its progress southwards, yet it is stated that it was carried from Mombassa to Lamoo by small craft, working along the coast in January, 1870, and from Mosambique to Ibo in the same month the following year, by a dhow, both against the north-east monsoon when at its height. We do not question the accuracy of these descriptions, but they show how cautious we should be in accepting such general statements as that the extension of cholera on the East African Coast is limited by the prevalence of the monsoons, and the inference from this, that therefore the disease must be carried by the traders. Notwithstanding these

slight blemishes, in taking leave of Dr. Christie's work we have again to express our appreciation of the assiduity he displayed in collecting his evidence, and to thank him for having made a very interesting and important addition to the history of cholera.

Many of our readers may remember that a long and elaborate report, by Mr. Radcliffe, on the outbreak of cholera at Mecca in 1865, including its diffusion that year both in the eastern and western hemispheres, appeared in 1866, in the Appendix to the Eighth Annual Report of the Medical Officer of the Privy Council. Compiled so soon after the occurrences, notwithstanding the valuable sources of information open to him, and the care with which the details are arranged, it was not to be expected that the author could draw up an account of the circumstances accompanying the spread of the disease so complete as not to require farther additions and emendations, as new facts, or more precise evidence, came into his possession. The Report mentioned at the head of this article repeats the main facts mentioned in the original as regards the outbreak at Mecca, with some important additions, which show that this was not an isolated manifestation, but merely one of a series embraced in a much wider diffusion. The course of the disease in Europe from 1866 to 1869 follows as a natural sequence to the diffusion in 1865, and its outbreak in Persia, Mesopotamia, Syria, and Asia Minor, in Eastern Africa, Abyssinia, Egypt, Northern and Western Africa, with its extension to the West Indies and United States are all recorded. In a subsequent section the recurrence of cholera in Russia in 1869, and its course in Europe up to 1874, inclusive, is narrated. A series of maps showing the general distribution of cholera, year by year, from 1865 to 1873, is attached to the Report; these are of great advantage in enabling the eye to take in the general relation of the different outbreaks, and in suggesting connections between them, which require to be wrought out by a further comparison of their details.

In compiling this work, Mr. Radcliffe has spared no trouble to render the materials at his disposal available in a compendious form, and any of our readers who may be investigating the spread of cholera will find it of great assistance.

We cannot go at any length into this extensive body of details within the space of a single article, but may notice some of their bearings on points at present under dispute. It will be remembered that the Conference which assembled at Constantinople in 1866 attributed the outbreak of cholera at Mecca, in 1865, to importation by the ships "Persia" and "North Wind," from Java and Singapore; it now appears that the disease was at Sana, the chief town of Yemen, in the end

of 1864, and Mr. Radcliffe gives a summary of the evidence on that and collateral manifestations in the following terms:—

1. "The infection of Mecca and the Hedjaz with cholera in 1865 was part of a wider infection, including Yemen and Hadramaut, in Southern Arabia, and the Somali country in East Africa.

2. "The infection of Yemen, Hadramaut, and the Somali country preceded the infection of the Hedjaz.

3. "The infection was a continuation of the epidemic movement of cholera westwards, which, beginning in the Lower Provinces, Bengal, in 1863, had extended the same year through the Central Provinces into the Bombay Presidency, and spread along the whole western sea board of Hindostan in 1864 and beginning of 1865.

4. "Although the details of transmission necessarily escape observation, it is to be inferred that the infection was carried seawards during the north-east monsoon, in the ordinary course of native traffic, and towards the close of 1864 (if not at an earlier period of the diffusion) was imported into Yemen, Hadramaut, and the northern Somali country" (p. 24).

Other six paragraphs follow, but as those just quoted contain the principles we are concerned with at present, while the others merely comment on them, it is unnecessary for our purpose to do more than refer to them here.

The first three of these conclusions contain a fair statement of the facts, to which no objection can be taken; the fourth is a matter of inference from these facts, upon which there is great difference of opinion, and which cannot be accepted without a more minute examination of the evidence than it has yet met with. Indeed, writers who adopt the theory that man himself carries the germ of cholera with him, differ among themselves materially in their estimate of his instrumentality in diffusing it; thus Mr. Radcliffe, referring to his report of 1866, says:—

"The report was chiefly successful in showing that no more mysterious agency was concerned in the rapid diffusion of the disease from Alexandria than steam navigation. All the seaport towns earliest attacked were in direct communication with that city; and in every instance where the beginnings of the irruption had been investigated with any precision, there importation was obvious or so highly probable as to be well nigh certain" (p. 12).

On the other hand, Mr. Simon, in his Supplementary Report of 28th July, 1875, enclosing that of Mr. Radcliffe, now under consideration, expresses himself in the following terms regarding the causes of the active diffusion of cholera:—

"Regarding cholera in India, apparently two sets of conditions have to be distinguished; (a) the conditions of the disease's endemic prevalence, in larger or smaller quantity, in a limited portion of the

Indian continent, and (b) the conditions of its occasional spread over areas indefinitely larger.

"That under the latter head must be counted *at least in part* the same property of contagiousness, and the same relation of this property to circumstances of filth, as we know to be the essential condition for the extension of the disease in Europe, is an opinion which European pathologists find it impossible not to entertain" (p. 9).

The italics in this quotation are ours, and if for "as we know to be the essential condition" be substituted "as may ultimately prove to be the essential condition," we would accept Mr. Simon's views not only as embodying the conditions which determine the epidemic extension in India, but its pandemic diffusion elsewhere as well. It is clear Mr. Simon contemplates the operation of other factors than those connected with individual men and their immediate surroundings, and necessarily factors of more general operation, and as, when we find a result follow the operation of more than one factor, we cannot determine how far it is due to that of more limited influence until an approximative determination, at least, has been reached of the power of the more general one, we deem the alteration of his words, just proposed, as absolutely required in the present state of our knowledge on the subject. The real points at present in want of elucidation are, we conceive, the nature and mode of operation of these more general factors (for there may be more than one), and their influence in favouring the development of cholera when in combination with those factors to which Mr. Simon more particularly alludes, or with others which affect the individuals only who are attacked.

Mr. Radcliffe offers the following general observations as embracing the main points set forth in the detail of the facts connected with the diffusion from Mecca, through Alexandria to Europe, which we deem it advisable to quote at length.

1. "That the epidemic invaded Europe at spots, and at such spots only, as were in the most frequent and directest communication with Alexandria.

2. "That in no instance did the invasion of the disease occur before there had been abundant opportunity for infection of the population invaded, assuming the disease to be infectious.

3. "That in several instances, *e.g.*, at Constantinople, Pistoia, Valencia, Altenburg, the introduction of the disease and subsequent spread in direct relation to the case or cases introduced was obvious.

4. "That in other places, *e.g.*, Marseilles, Borch, Malta, Ancona, Southampton, the inference of introduction of the disease, as the starting-point of prevalence is, from the preceding and concurring

events, as certain as if the different steps had been as clearly observed as in the places previously mentioned.

5. "That these conclusions apply equally to the places secondarily invaded, as Kustendje, the Danubian ports, and Odessa, from Constantinople; Gibraltar from Malta; Theydon Bois from Southampton.

6. "That the facts recorded of Marseilles and of Malta would appear to indicate that there is a period of the disease, as it is about to become prevalent or is becoming prevalent in a locality, when it is peculiarly transmissible, or is liable to be transmitted unrecognised. The transmission to Malta and Marseilles took place almost certainly before the existence of cholera in Alexandria had become officially recognised. Some of the apparent mystery of the mode of transmission in these instances no doubt, depends upon imperfect observation. But the transmission to Southampton, and through Southampton to Theydon Bois, shows how much we have yet to learn of the obscurer modes of transmission of the disease, or, in other words, of its slighter, perhaps unrecognised, and not fatal forms. The introduction to Borchl forms in some sort a parallel to that of Theydon Bois, the introducers in both cases having passed rapidly through a district in which cholera was about to become prevalent. Other instances of the same class are recorded in this report.

7. "The conclusion here arrived at or suggested from the phenomena of the invasion of Europe are the same as those derived from the phenomena of the invasion of Egypt. Similar conclusions follow from the invasions I have still to describe, each separate invasion adding to the force of the conclusion derived from other invasions" (p. 56, 57).

When examined in detail, we do not think the facts relied on by Mr. Radcliffe bear him out in these conclusions in every case, as we now propose to show. The Conference at Constantinople in 1866 emitted the opinion that the "time which elapses between the moment at which the individual receives the cholera infection and the commencement of the premonitory diarrhœa, or of confirmed cholera, does not exceed a few days (elsewhere more precisely limited by the words "a week at most"),¹ and the Conference at Vienna, in 1873, adopted this conclusion. On the other hand, Dr. Bryden, from Indian experience, informs us "that removal from an infected spot will almost to a certainty be followed by the cessation of cholera within the ninth day, and possibly much sooner."² We conclude, therefore, that an individual, or any number of persons, who have been exposed to the causes of cholera in any locality,

¹ Abstract, by Dr. Seaton, of "Proceedings of the International Sanitary Conference at Vienna," 'Report of Medical Officer of Privy Council,' N.S., No. v, p. 208.

² 'Cholera Epidemics of Recent Years,' &c., p. 203.

should manifest the disease within nine days of their leaving that locality, if its appearance among them arose from infection contracted there; further, if a number of persons have been exposed, and numerous cases of the disease result, the Indian experience shows that the intensity of the outbreak occurs from the third to the fifth day after exposure. It does not follow, however, that every or indeed any case of cholera which may have presented itself among these persons before the ninth day after having left the locality in question must be referred to infection contracted in it; they may have been exposed to other sources of infection subsequently, to which the disease was due, a distinction that must not be overlooked.

With the rapid communication by steam vessels, which existed in 1865, it is obvious that persons who had been infected at Alexandria might thus reach other points of the Mediterranean before cholera became developed in them, and the appearance of these cases was to be looked for precisely at such spots "as were in the most frequent and directed communication with Alexandria," that is, at places to which the period of transit was shorter than the period of incubation, and the traffic so large as to afford the greatest number of chances of infected individuals being included in it, but even with all these chances in their favour the number of infected individuals seems to have been remarkably small.

In examining particular instances of supposed importation, it must be borne in mind that the evidence in favour of it is often so mixed up with that pointing to the operation of local circumstances that the influence of the one cannot be separated from that of the other, and, consequently, such instances can neither prove nor subvert either view. The case of the outbreak at Constantinople, mentioned by Mr. Radcliffe in the third observation, we conceive is of this nature; the case of Valencia is deficient in details, for the first person said to have been attacked, on 8th July, had come from Alexandria by way of Marseilles, but the dates of his leaving both places are wanting. In the cases of Pistoia and Altenburg, however, the circumstances are more fully detailed, and deserve our attention.

As to Pistoia, it was supposed to have been infected through Ancona, so we require to notice what had been going on there for some short time before. On 17th June the Steamship 'Principe di Carignano' arrived at Ancona from Alexandria, with sixty passengers. She sailed from the latter port on the 12th, with a foul bill of health, the first that had been issued there for this epidemic, and, on arrival, was placed in quarantine for seven days, and the passengers were transferred to the lazaretto; no disease seems to have followed. How many

vessels arrived from Alexandria subsequently (if any) is not mentioned, but on the 3rd July there seems to have been one carrying thirty-eight passengers; these went to the lazaretto, from which they were liberated on the 9th. Mr. Radcliffe informs us—

“Persons detained in the lazaretto would appear to have had the privilege of sending their linen into the town to be washed, for it is to this custom, and especially to the washing of linen belonging to individuals from Alexandria then under detention, that the case of the 7th of July is attributed in the ‘Report of the Sanitary Conference’ of 1866. The evidence upon this subject, however, so far as it has come under my observation, simply goes to prove that the quarantine was illusive” (p. 49).

Now, as to the Pistoia case, a woman who had arrived from Alexandria on 3rd July remained in the lazaretto at Ancona from that date till the 9th. On the 10th, while travelling to Pistoia, she was attacked, and died the following day with all the symptoms of Asiatic cholera. The date of this woman leaving Alexandria is not mentioned, but if we allow the same number of days for the voyage as occupied by the *Principe di Carignano*, viz. five, she must have sailed on 28th June, and the disease declared itself twelve days thereafter, thus considerably exceeding the period of incubation mentioned above. We are not informed whether any disease occurred in the vessel from Alexandria, or among the passengers while in the Lazaretto, but from the silence on this point it may be inferred there was none, in which case the period which elapsed between this woman leaving Alexandria and the appearance of the disease gives us no alternative but to conclude she became infected subsequent to leaving that place, where the evidence does not enable us to decide, but enough has been mentioned to show that the epidemic at Pistoia cannot be connected with Alexandria through her.

Regarding the Altenburg case, Mr. Radcliffe states—

“On the 27th August, 1865, a woman, E—, sickened of cholera in Altenburg, and died on the 29th, who had reached that town from Odessa, on the 24th of August. She had started on the 16th of August, six days after the beginning of importation of cholera into the lazaretto of that port, and two days before the disease had been officially recognised as having appeared in the town; and she had with her a child suffering from diarrhœa at the time of starting, and which still suffered when she reached her destination in Saxony. Her journey occupied nine days and nine nights. The day E— died (the 29th), and in the same house, a sister-in-law was attacked with cholera in the evening and died on the 30th. The child was now removed by a sister of the woman E— to another part of the town,

where it died from exhaustion on the 31st. These cases were the beginning of the epidemic in Central Europe" (p. 54).

In the late Professor Parkes's 'Report on Hygiene' for 1865, in the 'Sanitary Report for the Army' for 1864, at p. 405, he gives an account of the same case taken from Pettenkoffer, in which there are some details not mentioned above. In this the woman is described as a lady, as having arrived at Altenburg, on the 25th (which agrees with the other dates mentioned above, which Mr. Radcliffe's day of arrival, the 24th, does not), and as being quite well up to the 27th. It appears, further, that she had passed along the Turkish frontier, where cholera existed, previous to her arrival at Odessa, but that she had no knowledge of cholera being at that place while she was there. The child from this account suffered from diarrhœa at Odessa, but became much worse after her arrival at Altenburg. The sister-in law had not been away from Altenburg. Pettenkoffer considered the local conditions at Altenburg in respect of the soil, the level of the subsoil water, and the impure condition of the soil itself, were favorable to the spread of cholera.

Are we justified in concluding that this lady introduced the disease? It is clear from her statement that she had not seen any case of cholera in Odessa, and the period which elapsed between her starting for Altenburg and her attack, viz., eleven days, is opposed to the assumption that she had been infected at Odessa. Further, had the child's complaint been choleraic diarrhœa, it should have developed its true character long before the party arrived at Altenburg, instead of which it only assumed an aggravated character after arrival there, and exposure to the conditions particularised by Pettenkoffer. In each case the evidence is against the view that the infection of cholera had been contracted at Odessa, and consequently, as there is nothing to show that it had been met with at any intermediate point, we have no ground to assume that it was communicated by those persons to others at Altenburg. On the other hand, the facts are quite compatible with this lady and her child having arrived at Altenburg, just as an outbreak was about to commence there, and there was sufficient time for the former to become infected, and, considering the long journey she had just completed, and that for the time she was fresh to the locality, there is nothing wonderful or unusual in her being the first victim.

It is admitted in the fourth observation that the evidence as to Marseilles, Borch, Malta, Ancona, and Southampton, is incomplete, and the conclusion that cholera was introduced is merely a deduction from the theory that attributes the spread of that disease to the poison disseminated in the discharges from the sick. Want of space prevents us going over all these in-

stances in detail, but we may state that the first case of cholera in Malta occurred in a soldier's child, in a detached position in the quarantine harbour, on 20th June, while the first case of cholera was landed there from the Wyvern, on her arrival from Alexandria, on the 28th. There had been several cases of diarrhœa among persons detained in the lazaretto, but none of them passed into cholera, nor were any fatal, but a month earlier (20th May), while these buildings were occupied as quarters by a portion of the 8th Regiment, eight men, a woman, and a child were attacked by nausea, vomiting, purging, pain in stomach, cramps, and coolness of surface, by cholera nostras, in short, the exciting cause of which was supposed to be the emanations from privies in the buildings; the occurrence of diarrhœa among the persons undergoing quarantine subsequently in these buildings, therefore, was not surprising.

At Southampton, again, Professor Parkes, after the most searching investigation into every circumstance connected with the appearance of cholera then in 1865, came to the conclusion that "the origin by importation is deficient in precision of evidence," in fact, though strongly impressed with the idea that it had been introduced in this manner, his efforts to trace the channel of communication completely failed.

The same general remarks apply to the fifth observation as to the fourth. The evidence as to Kustendji, the Danubian ports, and Odessa, is not sufficiently precise to enable us to arrive at a conclusion whether the outbreaks there were due to importation or other causes, even did our space permit of them being examined in detail. As to Gibraltar the facts are simple, the 2nd Battalion 22nd Regiment embarked on 5th and 6th July, in the *Orontes*, at Malta, the ship at the time lying in the quarantine harbour, opposite to and about 200 yards from an outwork where some children with cholera were quartered, and the drainage from which was said to run into the harbour near her. The *Orontes* left Malta on 6th July, and reached Gibraltar on 10th, when the regiment was landed, and encamped on the neutral ground, outside the fortifications. No sickness appeared during the voyage, nor after landing, until the evening of 18th July, when a man was attacked with cholera, and died the following morning. The wing of the corps to which this man belonged embarked immediately after for Mauritius, and reached its destination without more cholera. The other wing changed its camp to another part of the neutral ground, and on 31st July a man belonging to it was attacked with cholera, and died that evening; the same day a woman belonging to the wing was attacked, and died the following morning. The transport for this wing having arrived, it embarked on

1st August, and reached Mauritius without further attacks. Neither of these men had been near any cholera case in Malta, and, had the first imbibed the infection there, his attack as we have seen should have taken place most probably from the 9th to 11th July, and certainly before the 15th; while the two cases belonging to the other wing were not likely to have been in communication with him, and so to have been infected in that way at Gibraltar, and here too the period of incubation would have exceeded what it is found in practice to be. The evidence, therefore, is opposed to infection in Malta in the first case, or to the last two having been infected from the first; but is quite consistent with the three having contracted the disease from something they were exposed to at Gibraltar. The next cases which presented themselves there were in a family residing in a detached hut on the neutral ground, about a quarter of a mile south-east from the camp of the 22nd, with whom they were not likely to have had any communication; here a corporal of the 15th Regiment, in charge of the burial ground, and one of his children aged four years, were attacked on 3rd August, both of whom died, and seven days after another child, aged six, was attacked, but recovered.

The Theydon Bois instance was detailed in a former volume, but as the case is important, to save reference we shall repeat its prominent features here. Mr. and Mrs. Groombridge, from Theydon Bois, in Essex, had been seventeen days at Weymouth, when, on 25th September (1865), they returned home, proceeding to Southampton and London by the South Western Railway, and from London to Theydon Bois. Mr. Groombridge was seized with diarrhœa, sickness, and cramps, on 23rd September; these continued more or less through the 24th, but on the 25th he was so far relieved that he was able to travel. Mrs. Groombridge during the journey from Weymouth complained of pain in the back and some discomfort of the stomach and bowels, which she attributed to the shaking of the train; Mr. Radcliffe informs us in a foot-note, at p. 55, that he has reason to believe she may have visited the water closet at the station at Southampton during the halt of the train there, but it does not appear that either she or her husband left the station. On 26th September, the day after reaching home, she was attacked with diarrhœa, on 28th there was violent rice-water purging, with sickness and cramps, and alarming prostration, which on the 29th went into confirmed collapse. On the 30th reaction commenced, and secondary fever ensued, of which she died on the 11th of October. On the 30th of September two cases of malignant cholera occurred in the family, and others followed, leaving no doubt of its true nature. Mr. Groombridge

himself was attacked on the 6th October, and died early the following morning; during the progress of the complaint he declared that the diarrhœa, sickness, and cramps he had suffered at Weymouth were as violent as those he then laboured under.¹

The first case of cholera recorded at Southampton was on the 12th of August, in a young woman who had suffered from diarrhœa for a week. The next was in a man named Rose, who resided in Brewhouse Court, near the lower end of the High Street, five furlongs distant from the railway station; he had had diarrhœa from the 17th of September, which passed into cholera on the 22nd, and he died on the 24th. The third case was in a lad who resided at Weston Common, north of Southampton, and about a mile from the nearest point of the railway. This lad had diarrhœa on the 21st of September, which passed into confirmed cholera on the night of the 23rd, but he recovered. These were the only cases known up to the 25th of September, and as none of them were near the railway, the facts offer no support to the view that Mrs. Groombridge became infected during her transit through Southampton. Mr. Radcliffe himself made inquiries at Weymouth, Portland, and Dorchester, which Mr. and Mrs. Groombridge had visited, but was informed by the local medical practitioners, and others, that nothing approaching in character to epidemic cholera, choleraic diarrhœa, or autumnal cholera, had been observed in that district during September.² Trusting implicitly in the theory that ascribes attacks of cholera to infection derived from persons previously labouring under it, Mr. Radcliffe was disposed to think that there was some flaw in the history of the first case;³ and in a note the following occurrences are mentioned, which we are induced to repeat here solely with the view of placing before our readers every circumstance which has been deemed of importance in the case. These are, that Mr. Groombridge passed some time on the hills overlooking Portland Harbour, on the 23rd of September, while there was a strong breeze blowing from the sea; he noticed some vessels passing in with clothes hung up in the rigging to dry, and he became indisposed while watching them; and Mr. Clegg (who attended the family at Theydon Bois) asked "if infection might not have been blown from the ill-washed linen of a cholera patient;" but while it is the common practice at sea to hang clothes in the rigging to dry after washing, no proof was adduced that any of these vessels had cholera on board. The whole evidence, then, lends no support to the belief that Mr. or Mrs. Groombridge had been exposed to infection from a previous

¹ 'Transactions Epidemiological Society,' vol. iii, p. 86.

² *Ibid.*, p. 95.

³ *Ibid.*, p. 96.

case, either at Weymouth or its neighbourhood, or while passing through Southampton on their journey home. Whether Mrs. Groombridge's attack was due to influences at Weymouth or at Theydon Bois may be open to a doubt; for if she contracted her disease at the former it became developed within the recognised incubation period, and if at the latter, then the water used in the family was drawn from a well which had, for months, been contaminated by leakage from the soil pipe from the water closet, and into which the diarrhœal discharges of her husband, still with diarrhœa, consequent on his attack at Weymouth, and subsequently her own, were emptied. At this time, too, several cases of common sporadic cholera occurred in the Epping, and Harlow and Matchingam districts, quite unconnected with the Groombridge family. The whole weight of evidence then in this instance is, we contend, opposed to Mr. Radcliffe's conclusion, as expressed in the third, fourth, and fifth observations.

With regard to the sixth observation, we are not aware of any facts that prove that cholera is peculiarly transmissible, or liable to be transmitted unrecognised when about to become prevalent, more than at any other period of an epidemic. The theory which ascribes every case of what is called Asiatic or epidemic cholera to infection from a previous one, we have seen is not borne out by the facts observed in the early manifestations in different localities in 1865, the period in each outbreak where the evidence can be obtained in such a form as to permit of the influence of infection from persons being separated from the influences connected with localities, for, after the outbreak has continued for some time, it becomes difficult to separate these two, and in many instances advocates of either view, while they find a certain amount of proof in support of their own theory, cannot effectually exclude every thing that may favour that of their opponents. The commencement of an outbreak then is the only time we can make sure of finding the facts sufficiently distinct to permit of our tracing the results back to their causes; no doubt our means of investigation may yet undergo great development, and may then enable us to recognise distinctions where at present we can find none, and so open up new fields of view introducing certainty when we are now left to conjecture, but until we arrive at this development, if we desire to advance, we must be sceptical of a theory which has to fall back on the mystery of imperfect observation, and obscure modes of transmission, in opposition to facts observed with all the care and with every precaution our present knowledge demands.

Mr. Radcliffe admits that the infection of Yemen, and the Hedjaz, 1865, was merely a part of a wider infection involving Southern Arabia and the Somali country, in Eastern Africa;

the extension to Egypt and Europe, we contend, was merely a continuous development of the same epidemic, the direction of the diffusion, the localities occupied, and the rate of advance, being all determined like any other operation in nature by fixed laws. Were traffic, whether by sea or on land, the efficient means of diffusion, we should expect the disease to have spread from a point where it was active to others nearly in proportion to the amount of communication, and the shortness of the time required to reach them, but to this there were numerous and striking exceptions. In 1865, for instance, while the disease radiated from Alexandria to the eastern and northern shores of the Mediterranean, the return of pilgrims to the southern coast led to no manifestation anywhere, though many went by steamers, and they were the people most likely to convey it. Again, while Marseilles was recognised officially as being affected on 17th July, and up to 6th September cases had occurred in five departments adjoining that of Bouches du Rhone, in which it lies, yet notwithstanding the immense daily traffic from Marseilles to the north, the first cases reported beyond these five departments, along the line of railway towards Paris, were at Lyons, commencing on 11th September, and the next in a suburb of Paris itself on 22nd, having the populous town and districts between them untouched; facts such as these, and there are many such from Europe, from India, and from America, show that there is something else that regulates the geographical position and area of a cholera field, and that whatever influence traffic may exercise on the diffusion of the disease within this area, beyond its limits it ceases to be appreciable.

III.—Tomes on Dental Anatomy.¹

A FEW months since, when reviewing an admirable book on Dental Surgery, we took occasion to remark that there were few specialties making greater advances than that connected with diseases of the mouth and teeth, and it is now our pleasure to follow the review of Mr. Salter's work by one on 'Dental Anatomy' by Mr. C. S. Tomes. The past year has been notable for the successful efforts of those qualified practitioners who have devoted themselves to the study of dental disease to assert their position as well as to improve the educational standard of the test required from those who are content with the special qualification granted by the Royal College of Surgeons. For as for the inclusive degree of membership

¹ *Dental Anatomy.* By C. S. TOMES, M.A., M.R.C.S.

a preliminary examination in arts is now required, and we cannot conceive any step more calculated to bring a good class of men into the dental branch of the profession, most of whom, we trust, will now qualify themselves for the higher diplomas granted by the College of Surgeons. Some twenty years since but few surgeons had undertaken the study of dental disease, and the intimate connection of the teeth with other organs through the nervous centres seemed quite to have been forgotten, but now that, returning to the past, we seem to have come back to the days of Herodotus, who tells us that when he wrote physicians existed who devoted themselves to the special treatment of every portion of the body, the educated surgeon no longer deems the teeth beneath his notice, and indeed we find that so many medical men are now engaged in dental practice that it has been found feasible to institute a new society termed the "Association of Surgeons practising Dental Surgery," the qualification for admittance to which is a diploma entitling its possessor to be placed upon the medical register, and its excellent object "to encourage a higher educational standard, both general and professional, for those who may practise the special branch of Dental Surgery."

The more fully this object is attained the more completely will the miserable system of advertising pursued by many unqualified persons calling themselves dentists be discouraged—an end still more readily attainable if respectable journals would cease to give insertion to discreditable puffs which so much simply disfigure their pages.

The need of Mr. Tomes' book is unquestionable. The subject of Odontology, connected as it is with minute anatomy, is one fraught with peculiar difficulty, particularly with regard to the development of the teeth, and in spite of the aid given by the best appliances for observation, the investigations must be so minute and the action and method of application of various reagents upon the tissues results in so many different appearances, that it by no means unfrequently happens that the same tissues have been described under names so different that it is often a matter of great difficulty to determine the exact value of the discoveries of different observers, and this has been more particularly the case in comparing the work of German and French histologists, especially the former, the German being a language so rich in word formation that it is impossible for the ordinary student, without hard study, to compare and sift the results of work carried out by foreign *collaborateurs*. No successful attempt has been made to do this until now, and the author's task was rendered more difficult by the fact that the minute anatomy of the teeth has been worked at by so

many different labourers in the same field. Improved methods of observation have greatly altered the views formerly held as to the development of mammalian teeth, whilst considerable advances have been made in the minute anatomy of the dental organs of fish and serpents—a subject to which Mr. Tomes has devoted particular attention, working out the theories and observations of others, as well as supplementing them with materials the result of individual research. We are undoubtedly greatly indebted to German and French histologists for the painstaking care with which they have worked out the difficult subject of dental evolution and histology. Kölliker, Boll, Frey, Waldeyer and Neumann, having especially distinguished themselves in this direction, whilst in France we have to acknowledge the valuable researches of Robin and Magitot, who have indeed paved the way to the lucidity of more recent investigations, whilst in our own country the work of Salter, Beale, Owen, Tomes, and Huxley must not be forgotten. Mr. C. Tomes has made a most admirable condensation of the views of these several workers, and has also personally tested them in a way which gives especial value to his conclusions. His book commences with a chapter on the teeth of man, and inasmuch as it also deals with comparative Odontology, it may not be out of place to consider what a tooth is, so various are the forms under which it appears in different genera and to such different purposes are the teeth applied. They are invariably represented in human beings, birds, beasts, and fishes, by protuberances of an osseous character, this varying in consistency from the hardest ivory to a mere cartilaginous structure, but there is one common characteristic in all, whatever be their structure or their function, and that is that they are connected with the skin, that they are in fact “dermal appendages,” and that their osseous connections are not to be considered in any way in relation to their development. Thus the teeth of the shark or dogfish are only highly organised scales of the skin and are merely offshoots, or rather primarily inflexions, of the oral mucous membrane, which in the case of certain fishes has but a very indistinct line of demarcation from the external skin, the teeth or dermoid spines being scattered in every direction and confined to no particular bone, whereas in mammals they are, broadly speaking, entirely confined to the bones of the maxillæ. We may here say that Mr. Tomes seems to be a strong adherent of the Darwinian school and of the opinions of that school respecting the “archetype” theory, which so long reigned supreme, applying the same arguments to account for modifications of dental type as Darwin would apply to explain the forms of the beaks

of the Trochilidæ, or the various forms assumed by the flowers of the Orchidacææ. We must confess that we to a great extent sympathise with these views, although we should not be inclined to overthrow the "archetype" theory altogether, as there are some points in common between the two views, opposed though they seem to be, which are not absolutely in-harmouious. Whilst on the subject of Darwinism, it may not be out of place to mention the remarkable fact which Mr. Darwin adduces in proof of the connection existing between the teeth and hair, these being both developed in a similar manner from the skin. Thus, when there is a marked deficiency or exuberance of hair growth, the teeth will show a similar tendency to increased or limited development; in the hairless dogs of Turkey, their mouths are found to be almost edentulous, whilst in other cases excessive growth of hair, as in the case of Julia Pastrana, is accompanied by abnormal development of the teeth and papillary structures generally. Likewise the cessation of growth observed in the tusks of castrated animals is another proof that teeth are not incorrectly designated "dermal appendages." It is difficult, nay, seemingly impossible to trace the causation of these variations of growth in regard to development, and we must therefore be content, until our knowledge is far more perfect, to accept the facts merely as they exist.

The most important part of the book is devoted to the minute anatomy of the dental tissues and their development. As is well known, a tooth is made of three tissues, subject to certain special modifications of structure, viz., enamel, dentine, and cementum; the dentine being the predominating tissue, and surrounding the pulp, whilst the enamel in most cases invests with an adamantine casing that portion of the tooth which appears above the gum, though occasionally it extends partially or entirely to the base of the tooth. Mr. Tomes expresses himself as feeling hesitation in endorsing Prof. Owen's generalisation that "dentine is the most and enamel the least constant of dental tissues," his own investigations leading him to suspect that "rudimentary layers of enamel exist upon many teeth on which their presence has not been recognised," he having found that the formative enamel organs occur universally, quoting as examples the frog and armadillo, both of which have enamel organs, that of the former being as large as that found in the snake, though in neither case can it be absolutely proved that there is actually enamel upon the teeth, although the asserted existence of the enamel organ, verified by Prof. Turner in relation to the narwhal, would predicate the existence of a thin and rudimentary layer of that tissue. The cementum in mammalian

animals covers the roots of the teeth, and it is not impossible, as Mr. Salter suggests, that it is connected through the periosteum with the nutrition or even innervation of the teeth. In the case of snakes and other reptiles it is altogether absent.

With regard to the minute anatomy of formed enamel recent investigations have not led to any important discoveries. Its structure has been proved to be distinctly fibrous; the most frequent course of the fibres being from the dentine towards the surface, these pursuing either a tortuous and divergent, or perfectly straight direction, which is almost exceptionally seen in the manatee. In relation to these peculiarities Mr. J. Tomes observes that the course of the enamel prisms in the rodents constitutes a series of patterns which are constant to peculiar families, the most complex of them being found in the porcupine family in which the curves of the fibres are individually tortuous and not confined to one plane. With regard to the question as to the connection of the dentinal tubes with the enamel, we think that the doubts of Hertz and Waldeyer as to the passage of these into the enamel is quite set at rest; this arrangement being almost constant in marsupials, whilst it also occurs in certain genera of rodents and insectivora, of which the jerboa and soricidae might be instanced respectively. The fact as the author proves, is capable of actual demonstration, for if the enamel of marsupial teeth be dissolved away the tubes may be seen projecting from the edge of the dentine. The contents of these tubes has long been a moot point, and Mr. Tomes does not seem to have come to any very definite opinion upon the subject, but there is no doubt that the tubes are not merely bounded by the matrix of the tooth, but by definite walls which possess the character of indestructibility to a marked degree, similar to the parietal investments of the lacunæ and Haversian canals of bone. Of what the contents of these tubes consists is a question which has given rise to great varieties of opinion, some, like Salter, maintaining that it is a viscid material and not a distinct fibril, whilst Neumann, Boll, Frey, Kölliker, and others hold to the opinion that distinct fibrils exist—an opinion to which we are strongly inclined ourselves, although we are bound to pay the greatest respect to the opinion of so accomplished an observer as Mr. Salter. Not improbably these fibrils are more or less connected with the innervation of the tooth, as they are undoubtedly prolongations of the pulp into the tubes, and although no nerves have been actually demonstrated in connection with them, Boll has been able to demonstrate the existence of innumerable small fibres, in a pulp treated with a solution of chromic acid, passing up from a nervous plexus which lies beneath the *membrana eboris*, whilst the fact that

sensitiveness of dentine can be removed by the action of escharotics and anodynes would lead us to the inference that the contents of the tubes have a nervous supply. From the consideration of human dentine Mr. Tomes comes to that of the modification of this tissue termed vaso-dentine, in which the dentinal tubes do not radiate from a simple central pulp cavity, but are also connected with secondary vascular canals containing a tissue of a similar character to that of the pulp; these canals may be seen to great advantage in the Megatherium and is characteristic of the dentine of a large number of fishes, as also of certain mammals and reptiles. In fact, the arrangement is very similar to that of the Haversian canals in bone, and indeed, as Mr. Tomes remarks, vaso-dentine is sometimes so modified in structure, that in those cases in which the teeth become ankylosed to the maxillæ, as in certain fish, it is hard to distinguish the tooth substance from the bone itself. Osteo and secondary dentine also partakes more or less of the characters of bone; indeed, it is impossible to describe all the many varieties of dentine which exist, those characterising the teeth of the Varanus and Lepidosteus being the most remarkable, the latter of which is made up as it were of separate systems of dentine each provided with its own pulp cavity. A similar but more complicated arrangement is found in the tooth of Labyrinthodon, inaccurately described by Owen as consisting of radiating plates of dentine marked out by convoluted plates of cementum, the errors having arisen seemingly from the existence of lacunæ similar to those found in cementum, which, however, are by no means unfrequently discovered in the granular layer of dentine, and undoubtedly Mr. Tomes' assertion that the whole of the tissue of this tooth is made up of dentine without cementum is to be accepted as correct. The consideration of the histology of the crusta petrosa or cement brings before us that structure known variously under the names of persistent dental capsule, enamel cuticle, Nasmyth's membrane, &c. The minute anatomy and origin of this tissue has given rise to endless controversy. Nasmyth, who discovered it, considered it to represent the remains of the dental capsule, Huxley thought it to be the remains of the *membrana preformativa*, whilst Waldeyer held the opinion that it was formed by a portion of the enamel organ. It is doubtful whether this delicate membrane exists upon all teeth although on the crowns of human and mammalian teeth its presence is easily demonstrated as it can be detached from the surface of the enamel in the form of a sheet of extreme tenuity ($\frac{1}{200,000}$ inch), having the characteristic of great indestructibility and resisting the action of the strongest acids and alkalies. It not unfrequently happens that the cementum ex-

tends over the crown of a tooth, this insinuating itself into any natural pits or crevices existing on its surface in which the cementum being thicker at such points, encapsuled lacunæ have not unfrequently been found—a fact which is strongly in favour of Mr. Tomes senior and his son's opinion, that the membrane in question may be cementum, though we ourselves rather incline to the view which Mr. Nasmyth first promulgated, that it is actually the "persistent dental capsule." We now come to the most interesting chapters in the book, viz., those on development. The generally accepted history of dental evolution is that of Goodsir, which divided its stages under the names of "papillary," "follicular," and "eruptive." As a matter of fact a tooth germ is never formed superficially, but is always produced beneath the surface, sometimes at a considerable depth, as in the case of certain of the Reptilia.

Each germ consists of two parts, viz., the enamel organ and the dentine organ, the first being derived from the oral epithelium, the second from the deeper portion of the mucous membrane; and strangely enough Mr. Tomes' observations lead him to conclude that an enamel organ exists in the germs of all teeth quite independent of any subsequent formation of enamel. There is no doubt that the premisses upon which Goodsir founded his deductions were inaccurate, when he asserted that the origin of the tooth germ was a papilla of mucous membrane which subsequently became encapsuled. At the time when he made his observation microscopists had not the powers which they have now, whilst it can be easily understood how maceration, or steeping in spirit, might remove the superficial epithelium from a specimen and so give the appearance of a groove, like that described by him as the "primitive dental groove." The origin of the enamel organ was first correctly given by Kölliker in 1863, whilst Waldeyer, Magitot and others have gleaned much valuable information by their researches on the subject. Mr. Tomes has based his description of mammalian and human teeth chiefly upon their researches, whilst he himself has especially devoted his attention to piscine and reptilian teeth. We cannot do better than give the author's *résumé* of modern researches on dental development in his own words. At page 105 he gives a general account of the process thus:—

"The deeper layer of the oral epithelium sends down into the subjacent tissue a process the shape and structure of which is, in most animals, distinguishable and characteristic before the dentine germ has taken any definite form. This process enlarges at its end and, as seen in section, becomes divaricated, so that it bears some resemblance to an inverted letter Y, or it might more truthfully be

compared to a bell-jar with a handle; this constitutes the early stage of the enamel germ, while beneath it, in the mucous tissue, the dentine germ assumes its papilliform shape."

He then describes in detail the process of development in elasmobranch and osseous fishes, basing his description upon the researches of Huxley, Santa Sirena, and himself. The main difference in development of the elasmobranch fishes and the Teleostei is that in the former each germ is derived from the "next older tooth," whereas in the latter "each tooth arises independently and as it were *de novo*." Mr. Tomes' description of development in reptilia is particularly interesting, and are to a great extent based upon his own observations in which he has worked out and verified or disproved the theories of English and foreign observers who have been working in the same lines as himself.

The developmental process in reptiles does not differ materially, as far as the formation of the germs is concerned, from that seen in fish and mammals generally, but the chief interest is to be found in the relations of the constantly changing teeth to the germs which represent the embryos, so to speak, of their successors. Upon the lingual surface of the jaw in reptiles is found a region occupied by forming teeth only, for which Mr. Tomes appropriately proposes the name "area of tooth development." As the germs increase in size, they make a progressive movement towards the surface of the jaw, new ones being constantly developed beyond them. The way in which the ophidian reptiles swallow their food renders a renewal of their teeth frequently necessary, and a large number of them are perpetually developing themselves in reserve, the tooth sacs being placed vertically in a direction parallel with the surface of the maxillæ, being contained in a kind of pouch of connective tissue—a beautiful provision of nature to prevent their displacement during the expansion of the mouth. It seems that each enamel organ derives itself from its predecessor, whilst the dentine germs are formed independently of one another, although in connection with the enamel organs. As development progresses the tooth sacs lose their vertical position and become recumbent, the tooth not resuming its uprightness until it comes to the surface of the bone; there is thus no chance of the tooth piercing the mucous membrane of the snake's mouth by any accident, such as undue expansion and the like. The process of development in mammals is next fully explained, and the connection of the enamel organ, consisting from without inwards of an "external epithelium," a "stellate reticulum," a "stratum intermedium," and an "internal epithelium," with the oral epithelium, is effectually proved, the internal and

external epithelia meeting at its base; the latter maintaining its continuity through the medium of "the neck of the enamel organ" with the *stratum Malpighii* of the mucous membrane. Beneath this the dentine-germ is separately formed, first appearing as a mere opacity in the concavity of the enamel organ, which at the time of the formation of the dentine germ has undergone important structural development. The so-called dentine bulb ultimately forms the dental sac, which, in common with the former, has its origin from the submucous tissue, not like the enamel organ, from the superficial epithelium. We must commend the author's description of the minute anatomy of these organs to our readers, merely presuming that the essential part of the enamel organ is derived from the columnar cells of the internal epithelium, whilst the dentine is formed from the layer of odontoblast cells, sometimes incorrectly termed the *membrana eboris*. The cementum is probably formed from the internal portion of the dental sac, an assumption which seems justified by its exaggerated development in those animals whose teeth are covered with coronal cement. We next come to the subject of calcification, which may take place in two ways: firstly, by the secretion of salts within the forming structure; secondly, by the excretion of organic and inorganic constituents into the formative organ itself as in the case of bone and dentine. After examining the experiments of Rainey, Harting, and Ord in relation to the chemistry of osseous development, the views of Huxley, Kolliker, and Wenzel are criticised. Mr. Tomes in concluding his criticism upon the deductions of these observers, expresses his conviction that both the enamel and dentine are formed by *conversion* of the enamel cells, in the one case by that of the odontoblasts in the other, the three last-mentioned observers dissenting from the conversion in favour of the secretion theory in relation to the formation of enamel, Mr. Huxley basing his dissent upon the fact that certain reagents revealed the existence of a tissue (the *membrana preformativa*) between the enamel and the enamel cells, whence he argues that the latter have no direct agency in the production of the former. Mr. Tomes, in common with his father, opposing this view, maintains that the isolation of the youngest layer of enamel is owing to its chemical nature, which causes it like calcoglobulin and some other substances to be peculiarly resistant, whilst the occurrence of "Tomes' processes,"¹ the rigidity of the open mouths of the enamel cells, the pitted surface of the youngest layer of the enamel, the foraminated membrane which can be raised from it, and the relation of these facts to the

¹ *Vide* p. 142.

occurrence of the processes of the enamel cells, confirms, in his opinion, the correctness of the view held by himself and others. This controversy leads naturally to a consideration of the true nature of the *membrana preformativa*, a membrane which has had a protean existence, being known under names innumerable, having diverse positions assigned to it, and having its very existence strenuously denied and as strongly supported by observers whose claims to accuracy in histological research are worthy of the highest consideration.

We are inclined to agree with the suggestion that the papillary view of dental development was the cause of the attempts to prove the existence of this membrane, for, as is truly said, such a theory almost necessarily implied the existence of a basement membrane. According to the recent views of development it must be assumed to be either a secondary development of the superficial cells of the embryonic dentine germ and not a portion of the basement membrane of the oral mucous membrane, or else a process of the inflected epithelium. Its supposed existence has been one of the greatest stumbling-blocks to a clear comprehension of dental histology, some maintaining that it existed between the dentine and enamel, others between the enamel and the enamel organ, that is, external to the former, and others between the dentine and the pulp.

Huxley adopts the second view, thinking that the membrane which can be raised from the surface of the developing enamel was the *membrana preformativa*, and subsequently Nasmyth's membrane, a conclusion which would deny the enamel organ any participation in the development of the tissue after which it is named. Kölliker formerly held a similar view. Beale, Waldeyer, Wenzel, and others deny its existence altogether, whilst Messrs. Robin and Magitot, to whom we are so much indebted for their painstaking and accurate researches in dental histology, explain the appearance of a membrane over the pulp by the density of the gelatinous material of which the pulp is composed, this forming a firm nidus for the odontoplastic cells which project beyond it. This, becoming more or less corrugated, might lead to the supposition that an actual membrane exists. The contention is a difficult one to decide, but present information on the subject seems to militate against the opinion of those who still maintain the existence of a preformative membrane.

Next follows an interesting chapter upon the development of the jaw chiefly founded upon the modern and independent researches of Tomes and Humphrey, the former of whom carried out observations which confirmed the views of Hunter; whilst the latter has corroborated the views of both by his experiments upon the growth of the maxillæ in living animals; the test

points selected for these experiments upon the growth of the maxillæ are the tubercles giving attachment to the genio-hyo-glossi and genio-hyoidei, the mental foramen and the inferior dental canal, these undergoing inappreciable changes in position. Measurements taken from these points prove that the alveolar arch occupied by the temporary teeth corresponds to that occupied by the permanent teeth supplying their places, the increase in the size of the jaws being owing to additions to the surface of the bone which takes place concurrently with absorption and not owing to any interstitial increase, the absorptive process removing that excess of bone, which would otherwise reach from the bicusps to the condyles of the jaw, the traces of this action being found in the condyles and coronoid processes, those of growth existing on the developed surfaces. We had hoped to have seen some explanation given of the forces which cause the eruption of the teeth, but beyond remarking that it does not depend upon increase of length in the erupting organs, Mr. Tomes is silent on the question, characterising the "nature of the impulse" as an "unsolved riddle," we are inclined to think that it has some relation to the deposition of bone around and beneath the roots, absorption of the superficial portion of the alveoli taking place concurrently, so-termed *impacted* teeth remaining occasionally imbedded in the jaw owing to abnormal position and arrest of absorbent action. That portion of the work treating of the teeth of fishes and reptiles embodies the author's personal investigations, and after reading his concise and pointed criticisms upon the opinions of his *collaborators* in the field of dental anatomy, sometimes we think dismissing their theories somewhat too curtly, we regret that he has not entered more fully into the subject of his own researches. We have already alluded to the beautiful provision found in snakes for adapting the arrangement of the teeth to the great mobility of the mouth, and Mr. Tomes shows how this adaptation is also extended to the arrangement of the successional tooth germs, these being placed parallel with the bone instead of vertically as in other animals, a sac of connective tissue enveloping them and serving as an extra protection in addition to their position; thus, these teeth when they take their place as successional organs have "not only to move upwards but also to become erected," processes concerning which it is frankly confessed by the author that he is "quite unable to discern the means by which they are accomplished."

In viperine snakes a lost poison fang is replaced most expeditiously by means which are quite unique; there are, in fact, "two distinct chains of younger developing organs, all destined to keep the animal always supplied with *one* organ in a state of

efficiency." Upon the maxillary bones there is sufficient space for two poison fangs in close proximity to one another, only one fang being anchylosed to the bone at the same time. When one tooth is lost, its successor appears upon the vacant space by its side, not upon the place actually occupied by its predecessor, so that opposite sides of the maxillary bone are alternately occupied by the teeth in use. In the rattlesnake there are as many as ten reserve poison fangs arranged in parallel series, these succeeding one another from either series alternately. In the cobra a parallel series does not exist, and consequently the loss of teeth is not so quickly repaired in it as in the viperine snake—a fact which, as Mr. Tomes suggests, may account for the choice made by the snake-charmers of India, who invariably select a cobra in preference to other poisonous reptiles. Reptilian teeth are divided by a wide abyss from those of mammals, to which Mr. Tomes next devotes his consideration, confessing that he sees no chance of bridging over the gulf by any forms now known, though Professor Owen professes to have found some teeth in reptiles, discovered in the lacustrine deposits in South Africa, which he considers to be decidedly analogous to those found in mammalia, proposing for them the name of Theriodonts, but Mr. Tomes does not consider the evidence brought forward as to this connection final or conclusive.

The book is concluded by a clear and lucid epitome of comparative odontology. We are not called upon to extend our criticism to this portion of the book, as it only represents a concise account of that which is already known in relation to this subject. Mr. Tomes does his best to point out the affinities between the different orders and genera which he describes, and evidently has not as yet come to any decided opinion upon the original form or type of the mammalian tooth, though he doubts not that all the teeth in the jaw of a mammal have been derived from a "single form." The perusal of his book has been a labour of pleasure and profit, for although it possesses no great amount of originality, especially where we should most have expected it in relation to Mr. Tomes's own researches, he has so thoroughly and conscientiously worked out and sifted the investigations of fellow workers in the same field, that his book must be of great service to all Odontologists. His style is peculiarly perspicuous and pointed; it has no tendency to exaggeration of language, and indeed is peculiarly fitted to the elaboration of a subject like that to which it is applied. On the whole, the arrangement of the contents of the book deserves credit, though we think that his disposition of the types which he selects might have shown the gradations between them rather more clearly. The illustrations are worthy of all praise, and, speaking generally,

most of the criticisms upon the views of others are fair and unbiassed, though, as we have before observed, in some few instances, we think that the investigations and conclusions of at least one investigator in dental histology have met with rather less consideration than the position of their authors deserved; this doubtless arises from the firm faith of Mr. Tomes in the views of himself and those whom he supports. Taking the book as a whole we think that its compilation reflects the greatest credit upon Mr. Tomes, inasmuch as it supplies a well-recognised want, and will doubtless be the means of extending odontological research in this country; for histologists will now be able to conduct their labours upon the basis of a most conscientious and painstaking digest and criticism of all that has been recently written by European workers on a subject of such vast importance in the comprehensive and rapidly extending science of comparative anatomy, whilst the book itself cannot but serve to elevate the status of dental surgery which now reckons amongst its ranks so many distinguished practitioners in medicine and surgery.

IV.—Hammond on Diseases of the Nervous System.¹

THIS large work, which comes to this country fortified by the prestige of a sixth edition and the reputation of an author who fills some of the most important offices in New York in connection with his specialty, and whose opportunities for the study of diseases of the nervous system have, therefore, been very great and have evidently been largely utilised, naturally raises in the mind of the reader anticipations of high excellence,—anticipations which, however, are very inadequately realised. There are indeed throughout the book evidences of great industry and of wide clinical experience, the records of cases being often of great interest and value; but as a rule the symptomatology is meagre and ill-digested, and the space which might usefully have been appropriated to this part of the subject is occupied by lengthy dissertations on the history of the disease and by speculations in pathology, sometimes interesting, but often premature and unprofitable. Thus, of thirty-nine pages devoted to aphasia no less than sixteen are occupied by a history of the disease from the times of Isaiah, Thucydides, and

¹ *A Treatise on the Diseases of the Nervous System.* By WILLIAM A. HAMMOND, M.D. Sixth edition. New York, 1876.

Pliny to the present day. Eight pages are given to the history of spinal anæmia and only five to the symptoms. The sections on diagnosis are as a rule faulty and show considerable haste and want of care in their construction. In the diagnosis of tubercular meningitis from typhoid fever, for instance, stress is laid on the tenderness in the right hypogastrium and on the absence of constipation in the latter disease, both of which are very untrustworthy symptoms, being often absent at that period at which diagnosis is most difficult; and no mention is made of the lengthened premonitions, the slow irregular pulse, the local paralysis and spasms, and the swelling of the optic nerve which are absolutely distinctive of tubercular meningitis as contrasted with typhoid. In the diagnosis of cerebral tumour from epilepsy it is said (p. 300) that "in tumour the convulsions are seldom accompanied by mental weakness and never by periods of actual unconsciousness." Dr. Hammond can scarcely mean as his words imply, unconsciousness during the convulsion, for if so he is undoubtedly in error, and even at variance with his own teaching elsewhere (p. 294, where he says "sometimes consciousness is not lost"). He probably means periods of prolonged unconsciousness after the convulsion, during which the patient is not in stupor, but is able to perform actions of considerable complexity and even to appear quite conscious, such as are related on pp. 670-3 as occurring in epilepsy. In his chapter on cerebral tumour there is a most important omission. Not a word is said of the very great similarity between certain cases of chronic Bright's disease and cases of cerebral tumour. Bright's disease is not even mentioned in the section on diagnosis. It is perhaps not sufficiently well known that Bright's disease may simulate cerebral tumour so closely that the diagnosis between them may for a time be impossible. There is the severe pain in the head which drives the patient almost frantic, and which is relieved by no remedy; there is persistent uncontrollable vomiting, which bears no relation to the ingestion of food; there may be one-sided convulsions, and not uncommonly there is a form of optic neuritis which even an accomplished ophthalmic surgeon cannot distinguish from that of cerebral tumour. And here we have to notice the surprisingly little mention that is made of the ophthalmoscope throughout Dr. Hammond's work. In acute meningitis it is not even mentioned; in tubercular meningitis it is stated that M. Bouchut finds peri-papillary congestion, dilatation of the retinal vessels, and deformation of the papillæ; but it does not appear that Dr. Hammond has found these appearances; in cerebral abscess it is stated that there are congestion and infiltration and subsequently atrophy, the result of neuritis, but there is no mention

of the fact that the neuritis may pass off leaving the disc apparently normal and without a trace of atrophy. In cerebral tumour optic neuritis is barely mentioned. In the chapter on apparatus Dr. Hammond says :

“Ophthalmoscopic examinations require the observer to possess a very thorough acquaintance with the anatomy of the eye and also with the science of optics. Unless these qualifications are enjoyed it will be much better to send the patient to a competent ophthalmic surgeon for an examination than to rush to hasty conclusions based on the most thorough ignorance.”

It is certainly advantageous for a physician to know the anatomy of the eye and the science of optics, but most certainly such a knowledge is not in the least indispensable to a medical ophthalmoscopist. A very little practice will enable any one to recognise a normal disc and to diagnose the chief changes to which it is liable, and even this small amount of knowledge is of enormous value in the recognition of disease. An ophthalmoscopic examination is just as essential for the diagnosis of *every* case in physician's practice as auscultation is, and ought to be just as much a matter of course and of routine.

In the matter of treatment Dr. Hammond is very sanguine. In hemiplegia from cerebral hæmorrhage, even when of old standing, he recommends subcutaneous injections of strychnia, and says also that amendment almost invariably follows the use of electricity. There is indeed scarcely any disease in which electricity in some form or other is not said to be beneficial, and it is even recommended to treat aphasia by the application of galvanic or faradic currents to the tongue!

The first disease considered by Dr. Hammond is cerebral congestion, of which he gives a description such as is commonly found in text-books on nervous diseases. The affection is said to be of two kinds—active and passive, or arterial and venous, and a sufficiently elaborate description is given of the different varieties of each kind. It is not a little singular that these accounts continue to be given by clinicians in spite of the repeated warnings of pathologists of the extreme untrustworthiness of cerebral congestion as a post-mortem appearance. It is very common after death, they say, as a result of the mode of dying simply, and it is not seldom absent in those cases in which its presence has been diagnosed during life. The reason why cerebral congestion is so often found after death with “cerebral symptoms” is that patients in coma die by asphyxia, and congestion of the brain is invariably found after death by asphyxia (Wilks and Moxon). There can be no doubt that the amount of blood in the brain is subject to variation, probably great

variation, and that a great increase in its quantity, especially if it occur rapidly, may, and even must give rise to symptoms; but that the groups of clinical symptoms described by Dr. Hammond and generally accepted under the heads of the apoplectic, paralytic convulsive, soporific, maniacal and aphasic forms of cerebral congestion are really due to hyperæmia of the brain, there is no satisfactory evidence to show, and there are very weighty reasons to deny. Let us first take the premonitory symptoms and see how far they can be accounted for on the hypothesis of cerebral congestion. On the psychological side (Dr. Hammond includes them among the *physical* symptoms of disordered cerebral action), there occur headache, vertigo, noises in the ears, and flashes of light before the eyes. Setting aside preconceived notions, not one of these can be said to indicate cerebral congestion. Niemeyer says very truly of headache that it is a very frequent symptom in all cerebral diseases, and if, as is most probable, vertigo be a motor symptom, it will be due to a local lesion and the subsequent remarks on local lesions will apply to it. Among the physical symptoms properly so called comes first heat of the head, but Dr. Hammond does not say whether this is a subjective sensation, which would of course be of little value, or a thermometric observation. Dr. Reynolds says that the head is hotter than the cheeks; but the temperature of the scalp in comparison with that of the face can hardly be considered an index to the blood supply of the brain, since both the former are supplied by branches of the external carotid. Dr. Hammond avers that the arteries of the retina are increased in number, diameter, and tortuosity, and that occasionally the optic disc is found congested. Now, with regard to this symptom, setting aside the numerous cases in which it is present without any of the general characteristics ascribed to cerebral congestion, we have to ask whether the cases of cerebral congestion in which it obtained were not really cases of gross intracranial disease. The symptoms of some of the forms of cerebral congestion are, as we shall show presently, absolutely indistinguishable from those of visible brain disease, and the fact that the congestion of the disc and the general symptoms of the malady may entirely disappear does not in the least militate against the hypothesis of a gross lesion. Much stress has been laid on the throbbing of the carotids and temporals as an evidence of increased flow of blood to the head, but it is forgotten that this symptom does not indicate that the artery is overfull; in that case, its tension being great, it would yield but little and slowly to the distending force of the systole. The throbbing indicates that the arterial tone is diminished, the artery is relaxed, flaccid, and yields readily and much to the sys-

tolic wave; it has the same significance as a long vertical upstroke in a sphygmographic tracing.

The rest of the premonitory symptoms are disorders of mind, of motion, and of sensation (in the ordinarily accepted use of the latter term). The first series, those due to disorders of mind, are of a very general character. There is drowsiness combined with sleeplessness, inaptitude for mental exertion, emotional instability, hallucinations, illusions and delusions. Of the symptoms due to disorders of motion and sensation it is most important to notice that the great majority are local. There are local spasms, local pareses, local numbness, local anæsthesiæ; and there are disturbance of speech and vertigo, which are evidences of local lesions. That these local disturbances of function indicate lesions of the nerve centres just as strictly localised there can be no doubt. It is not conceivable that slight general damage to the whole brain should produce twitching of one side of the face or numbness of one leg. Omitting, then, these symptoms, it is to be noticed that all the rest of the premonitions are of an exceedingly general nature. They are all very common symptoms and are precisely the symptoms which precede the outbreak of any acute disorder. Compare the following abstract of Dr. Russell Reynolds' description of the premonitory symptoms of cerebral congestion with Niemeyer's description of the initial symptoms of typhoid fever. Dr. Reynolds says :

"There is diminished intellectual power, the individual talks at random, using wrong words; sleep is heavy and disturbed by dreams. Hearing is defective and there are rumbling noises in the head; the sight is dim and black specks appear before the eyes; there is giddiness and a feeling of oppression and fulness in the head with 'stupid headache.' The limbs feel heavy, and there is often numbness in the toes and fingers. There are redness, and often dusky redness of lips, conjunctivæ, face, and scalp. The pulse is slow and laboured or quick and feeble, the tongue foul, the urine small in quantity and often loaded with lithates. The bowels are constipated."

Niemeyer says of typhoid patients :

"They feel excessively fatigued and weak, complain of heaviness and numbness in the head, occasionally also of headache. These symptoms are followed by flashes of light, tinnitus, deafness, pain in the muscles, trembling of the extremities on motion. The patients usually lie in a state of apathy, talk in their sleep, and while awake have muttering delirium. The pulse is large, full, and soft, and beats about 100 to the minute, the urine is scanty and of high specific gravity."

The premonitory symptoms then are those of general consti-

tutional disease, and are by no means specially indicative of cerebral congestion.

Passing on now to the fully developed malady, its symptomatic manifestations are said to be very various. Thus, Dr. Reynolds describes four forms, Dr. Hammond six, and Andral no less than eight. Dr. Hammond's varieties include the apoplectic, paralytic, convulsive, soporific, maniacal and aphasic forms; and of each form it may be positively affirmed that the symptoms described by Dr. Hammond are *absolutely indistinguishable* from the symptoms of well-known diseases marked by distinct anatomical lesions recognisable after death; and that the sole difference between the symptoms of cerebral congestion and those of the diseases in question is one of degree or severity. In the cases of "cerebral congestion" the symptoms are of mild severity, of transient duration, and therefore are not attended by grave consequences. The patient recovers, temporarily at any rate, and the diagnosis of cerebral congestion is considered proved. But the very same patients, Dr. Hammond admits, may have the attacks repeated again and again, with increasing severity, and when at last death occurs an abscess or a tumour may be found in the brain. This is not so in every case, it is true. A certain proportion of cases die and on autopsy nothing is found. It is surely better to admit that in these cases the cause of death is unknown than to check inquiry by calling it "cerebral congestion."

In order to examine more fully this position, that the symptoms of "the fully developed form of cerebral congestion" are really due to slight degrees of coarse intracranial disease, let us take in detail Dr. Hammond's description of the apoplectic form, to which he says about half the cases of cerebral congestion belong. The onset, he says, is generally sudden. The patient is perhaps walking in the street when he staggers, loses consciousness, and falls. The loss of intelligence and sensibility is, however, rarely complete, and may last but a few minutes or even seconds, though sometimes continuing for several hours. Paralysis is always present for a time; one limb only may be affected; it is never complete. The face is rarely involved, and the patient, though answering briefly when addressed in a loud voice, speaks indistinctly and with difficulty. The respiration is slow, but rarely stertorous, and it is not often that there is puffing of the lips and cheeks. The sphincters generally retain their power. The patient is rousable in so far that he will move his limbs when pinched and even answer a question put in a loud voice. The attack gradually passes off, the paralysis usually disappearing. It is common for a patient to have repeated seizures.

This is an exact description of a case of small cerebral hæmorrhage. The onset is precisely that of cerebral hæmorrhage; the paralysis indicates a *local* lesion; the degree of the paralysis indicates that it is a small or at all events a comparatively trivial lesion; and the position of the paralysis indicates the same thing, for if the symptoms be compared with the account given by Dr. Hughlings Jackson in 'Reynolds' System' of the order in which the parts suffer in hemiplegia (from cerebral hæmorrhage), it will be seen that the parts paralysed are those which are always first and most affected in paralysis from clot. The face, Dr. Hammond says, is rarely affected; that is, the lesion is not grave enough, the paralysis has not travelled far enough. Again, the absence of stertor, the non-affectedness of the sphincters, the incomplete or transient loss of consciousness, the recovery more or less complete of the paralysis, all indicate that the lesion was not a grave one. Intensify these symptoms and the description becomes typical of large cerebral hæmorrhage. But if the paralysis pass off completely, it may be said, Does not that negative the hypothesis of a clot? Certainly not; no fact in pathology is better established than that paralysis from destruction of a small part of the brain may pass off so completely as to leave no recognisable trace. It is of course not implied that every case of "the apoplectic form of cerebral congestion" is due to cerebral hæmorrhage. Two cases are mentioned by Dr. Hammond in which death ensued and no hæmorrhage was found. But it is most strongly insisted that in every case local symptoms must be due to local lesion. The symptoms above described would with slight modifications apply to other forms of cerebral disease—to hæmorrhage from tumour, for instance, or to attacks due to acute changes in the neighbourhood of tumour or abscess; and in these cases the ophthalmoscopic evidence would depend on the existence of tumour, not of congestion. We may pass over the paralytic form, which differs from the last only in the absence of the incident of unconsciousness and the convulsive form, to which most of what has been already said will apply. As regards the maniacal form we may quote Dr. Maudsley:

"In what is called *mania transitoria*! the patient falls suddenly into a violent fury in which he raves and often exhibits destructive impulses; his face is flushed, his head hot, and there is plainly an active determination of blood to the brain; and in a short time the fury subsides and the man is himself again, scarce conscious or quite unconscious of what has happened. Was the rush of blood the primary and active agent in the production of the fury? Was it not rather secondary to the intense morbid or degenerate action of the nervous centre; the attack representing a sort of epilepsy and

the congestion taking place not otherwise than it takes place in the cord poisoned by strychnine?" [In which "the congestion" is plainly a secondary result of the directly poisoned nervous element.]

The aphasic form of cerebral congestion is characterised by the very sudden onset of aphasia with or without pain in the head, vertigo and confusion of mind. The description is precisely that of embolism in the region of the middle cerebral artery, the only difference being the rapid recovery, which, as has before been explained, signifies that the injury is a small one; the artery blocked is a small branch, or that the embolism has disintegrated. There is absolutely no other difference between this description and that of an ordinary case of embolism. Dr. Hammond mentions two cases in which patients were rendered suddenly aphasic by the administration of nitrite of amyl, which causes great congestion of the face, and presumably of the brain; but it is to be remembered that the circulatory disturbance caused by the administration of amyl nitrite is a condition very favorable for embolism. But the most convincing argument in favour of the hypothesis that the symptoms attributed to cerebral congestion are really due to coarse disease of the brain is found in the admission of Dr. Hammond, that "the principal phenomena [of the third stage of the malady] are those connected with secondary lesions, such as inflammation, abscess, softening and adventitious growths of various kinds." Dr. Hammond admits that these lesions are subsequently present in many of his cases, so that the point at issue narrows itself to this: when a series of similar symptoms occurs with gradually increasing severity until death, and then a definite morbid change is found, is it more philosophical to suppose that the whole of the symptoms are due to the gradual increase of the lesion discovered, or that the late symptoms only are due to this lesion, and the early ones, although similar, are due to a totally different one? This question is not merely a matter of speculative pathological interest; it has also a practical side of very great importance. Once it is recognised that "cerebral congestion" is an occurrence of extreme rarity as a cause of grave symptoms, and it will cease to be the practice to shave the head, to put a blister on the nape of the neck or behind the ears, mustard poultices to the calves of the legs and the pit of the stomach, to give quantities of calomel internally, and in other ways to worry every patient who has a small cerebral hæmorrhage.

The chapter on cerebral hæmorrhage contains records of some valuable facts, but there is a lack of scientific arrangement. The

symptoms are detailed with completeness but without order, and there is not sufficient allowance made for the difference in the nature as well as in the degree of the symptoms attending large and small hæmorrhages. The descriptions apply to large hæmorrhages only, probably because cases of small hæmorrhage are attributed by Dr. Hammond to cerebral congestion. Dr. Hammond does indeed mention cases in which the face, tongue, arm or leg respectively alone were paralysed, but English observers believe such cases to be very rarely due to cerebral hæmorrhage. Cases are very common in which paralysis of one of these parts is the prominent symptom, but in such cases it is believed that there is always more or less involvement of some other parts on the same side, the degree of this involvement depending partly on the size, partly on the position of the hæmorrhage. The teaching on the very difficult subject of diagnosis is rather meagre, and says but little on those most important cases in which no local paralysis can be discovered, but Dr. Hammond insists strongly on the very important fact that in some cases discrimination between cerebral hæmorrhage and drunkenness or other forms of apoplexy is quite impossible. It cannot be too strongly asseverated nor too widely known that in many cases of loss of consciousness a diagnosis cannot be made, and this is especially true of those cases—of people found in the streets by the police, for instance—in which no history can be obtained and to which a prominent publicity may be attached.

Cerebral softening is considered by Dr. Hammond separately from the lesions to which it is due, which seems an unscientific course, since the symptoms to which it gives rise are simply those of destruction of brain tissue, and cannot when separated from the clinical history of the onset be distinguished from those of destruction of brain tissue from other causes.

Dr. Hammond defines aphasia as “a condition produced by an affection of the brain by which the idea of language or of its expression is impaired.” There is a verbal exactness in this definition, but it is difficult to understand what is meant by “the idea of the expression of language.” The necessary distinctions between aphasic patients and those who are able to speak but will not, and also between aphasia and loss of power of articulation, are clearly described; and then follows a long account of the history and bibliography of the subject, after which are detailed a series of typical and instructive cases. Dr. Hammond then goes on to say:

“It cannot have failed to strike the reader that in all the cases of which hemiplegia formed a feature the aphasia was of the ataxic form; while, when there was no hemiplegia, the aphasia was

amnesic. In the one the individual was deprived of speech because he could not coördinate the muscles of articulation; in the other, because he had lost the memory of words. . . . The grey matter of the lobes presides over the *idea* of language, and hence over the memory of words. When it only is involved there is no hemiplegia, and there is no difficulty of articulation. The trouble is altogether as regards the memory of words.

"The corpus striatum contains the fibres which come from the anterior column of the spinal cord, and is besides connected with the hemisphere. A lesion, therefore, of this ganglion or other part of the motor tract causes paralysis of motion on the opposite side of the body. The cases I have detailed show, without exception, that the power of coördinating the muscles of speech is directly associated with this hemiplegia."

Dr. Hammond then quotes *in extenso* Professor Ferrier's views and exposition of Dr. Hughlings Jackson's views on aphasia from the 'West Riding Asylum Reports,' and, from his subsequent remarks, it is quite evident that Dr. Hammond has altogether missed the true comprehension of these opinions. As Dr. Hammond's is a clear statement of the common explanation of aphasia, it may be useful to contrast the two.

Dr. Hammond's explanation is that there are two centres:—one, in the corpus striatum, governs the movements of articulation; the other, in the convolutions, "presides over the *idea* of language, and hence over the memory of words." Lesion of the first centre causes "ataxy of articulation," "loss of the power of coördinating the muscles of articulation," and so produces loss of speech from a quasi-mechanical defect. Lesion of the second centre causes "loss of memory for words." The two affections are of different orders of phenomena: one is purely physical, the other purely psychical. The first thing that requires notice in this explanation is that it attributes functions of the extremest possible diversity to two parts which are alike in composition, continuous in structure, and intimately connected by position and blood supply. But this objection is comparatively trifling. There is also a grave confusion of physical with psychical phenomena—an error which of all others it behoves a student of cerebral diseases and mental affections to avoid. "The grey matter of the lobes presides over the *idea* of language." Now, what is meant by the grey matter presiding over an idea? Either it means that an idea is a species of force residing in the cells of the grey matter, or it is a brief and inexact method of expressing the doctrine that during the occurrence of certain physical changes in the grey matter certain mental states arise in the consciousness. We should not have thought of attributing the former of these meanings to Dr. Hammond, were it not that he speaks in

another place (p. 309) of the brain as "the chief organ from which the force called the mind is evolved." It is quite surprising to find a writer on diseases of the nervous system adhering to a view long since discarded by psychologists, and alluded to by one eminent thinker as belonging to the crude materialism of the savage. It is unnecessary to slay the slain by discussing this matter here, especially as it has recently been exhaustively considered in a contemporary ('Brit. Med. Journal'). If, however, it is in the latter sense that we are to understand the phrase, then there is this defect in the explanation: that while we are told that the inability to speak is due to loss of the function of a certain portion of the brain which has been destroyed, we are not told what that function is which is lost. To say that memory for words is the lost function is to repeat the error of confounding mental with physical phenomena. Even if "loss of memory for words" be the *mental* defect, it remains to be shown what defect in the functions of the nervous system this mental defect accompanies. In other words, what are those physical changes in the brain which occur simultaneously with the origination of ideas of words in the consciousness, and which produce those movements of the articulatory organs which result in external speech? It is at this point that the common explanation fails, and it is precisely this hiatus which is filled by the hypothesis advanced by Dr. Hughlings Jackson. The physical changes in question are called by Dr. Jackson the "anatomical substrata"¹ of words; just as the physical change which occurs in the brain when visual ideas arise in the mind are called the "anatomical substrata" of visual ideas; and so on of other mental processes. So that the question,—what physical changes take place in the brain when ideas of words arise in the consciousness? becomes, in Dr. Hughlings Jackson's phraseology, what are the anatomical substrata of verbal ideas? With regard to the nature of these anatomical substrata of words, there are two chief hypotheses. One is that enunciated some years ago (Jan. and April, 1869) in this Review by Dr. Charlton Bastian; the other is that enunciated by Professor Bain, amplified and applied to the phenomena of aphasia by Dr. Hughlings Jackson, and later by Professor Ferrier. These two hypotheses agree in this, that they both take as their starting-point that

¹ Strictly speaking, these changes are the *physiological* substrata of words, the term anatomical substratum being applied to the complex arrangements of nerve-cells and fibres which "represent" (see p. 317) the articulatory movements and in which these changes take place; but as the term "anatomical substratum" occurs so frequently in Dr. Hughlings Jackson's writings, and is such a purely technical and ill-understood expression, it has been thought better to retain it in this sense.

certain portions of the brain-centres—are in full physiological activity during outward speech, during the expression of speech by the articulatory organs; and that during internal speech—that is, the revival of words in the consciousness without articulation—there is a less degree of physiological activity of the same centres or of some of them. Dr. Bastian believes that the first thing that occurs in speech is an activity of the auditory perceptive centre, the mental side of which is that the memory of the word is revived in the consciousness.¹ In internal speech—in silent thought—this is the whole process, the activity goes no further. But when the word has to be spoken articulately, the physiological activity of grey matter is transmitted from the auditory perceptive centre to the motor centres for the articulatory muscles, and outward speech is the result. Thus it will be seen that, according to Dr. Bastian's view, there are two processes—a sensory process, which alone is essential to internal speech, and a motor process, which, when added to the sensory, gives rise to external speech, which therefore is an audito-articulatory, that is, a sensori-motor process. According to this hypothesis, the physical basis, the anatomical substratum of memory for words, consists in certain organically registered changes in the auditory perceptive centres.

According to the other hypothesis there is, during external speech, full physiological activity, or, as Dr. Hughlings Jackson expresses it, “discharge” or excitation of the motor centre for articulatory movements. (Other subsidiary processes may be simultaneously aroused, but these may for the present purpose be disregarded, as having more to do with the ideas expressed by the words than with the words themselves.) This motor centre is a complex organ, having different groups of cells and fibres which represent the movements of the articulatory muscles in various degrees of complexity. (By a nerve centre representing a movement is meant, that when the nerve centre energises or is discharged, that movement occurs or tends to occur.) Those groups or centres which are low down in the series, in or near the motor tract, combine the muscles so as to produce movements of the simplest, most general, and most automatic character; movements, that is to say, which are not only simple, but which are themselves component parts of many other more complex movements, and which therefore occurring

¹ It is not strictly correct to speak of the memory of a word being revived in the consciousness, for the idea of the word *is* the consciousness for the time being; but the expression is used here in order to emphasise the distinction between the physical and the psychical. Dr. Hammond believes that the memory of the word is revived in the *brain*, a verbal proposition which, uniting phenomena of different orders, has no corresponding mental state. Like the proposition Heat is square, it is unthinkable.

with great frequency become more definitely "organically registered," more prone to recur, attended by less voluntary effort, more automatic. The centres which are situated higher and higher above the basic ganglia combine the movements represented in the lower centres into other sets of movements, more complex, more special, and more voluntary; that is, into movements which require the co-operation of more numerous and more delicate muscles, which have a more definite individual character, and have therefore a more restricted capability of entering as component parts into movements of still higher character, and which, therefore, occurring seldomer, are less prone to recur, are attended by a stronger voluntary effort, are less automatic. For instance, the lowest centre of this series represents, we may suppose, simply the muscles of respiration, those forming what has been aptly termed the "coarse adjustment" of the larynx and those which open the mouth. Discharge of this centre will cause expulsion of the air from the chest past the vocal cords in their simplest position, and will produce a simple cry. That is to say, first a very simple movement, one which requires the operation of the fewest and coarsest of the articulatory muscles; second, a very general movement, one which forms a component part of every other act of articulation; and third, a very automatic movement, one which occurs easily with very slight voluntary effort. In accordance with this is the fact that it is the first articulatory effort made by the child, and the first which occurs in the ascending scale of animal life. Centres a degree more complex than this re-represent the movements of the first centre, and superadd to them a finer movement of the larynx and the simplest movements of the tongue or of the lips. Discharge of one of these centres will cause expulsion of the air from the chest past the vocal cords in a more accurate position, and so will produce a more melodious cry, which will be modified or broken by a simple movement of the tongue or lips. The inarticulate cry of the infant becomes da-da or pa-pa, and the gaa of the sheep becomes the bow-wow of the dog. That is to say, the movement represented by this higher centre is first more complex, it requires the operation of more numerous and more delicate muscles; second, it is more special, it has a more definite individual character, and has therefore a more restricted capability of entering as a component part into movements of still greater complexity. For clearly, the simple phonation produced by energising of the first centre enters into the composition of da-da and pa-pa—both of the lip and the tongue series, while each of these latter movements can enter into the composition of further developments of its own series only. And third, it

therefore occurs seldomer, and being consequently less automatic, it is attended by a stronger voluntary effort in its performance. Continuing this process upward, it is easily seen that the highest centres will represent elaborate movements, which will result in the formation of spoken words. When these highest centres, then, are fully discharged, words are uttered; and simultaneously with this purely physical process, ideas of words arise in the consciousness. In internal speech, when, that is, the word is only revived in the consciousness and not uttered, there is on this hypothesis a much feebler degree of excitation of the same (motor) centre. "When we remember a word there is a faint excitation in the highest centres for articulatory movements; when we remember and also say that word aloud, the excitation is stronger, and currents spread down to lower centres of movement and thence reach the articulatory muscles." On this hypothesis, therefore, the anatomical substrata of words both spoken and unspoken are motor processes.

According to this hypothesis, if there be a lesion "low down," *i. e.* in or near the basic ganglia, that nervous centre is destroyed whose discharge or physiological activity gave rise to simple movements of the articulatory muscles. But this is not the only effect of such a lesion. Since the "higher centres," those nearer the surface of the brain, whose discharge gives rise to more complex movements of the articulatory muscles, are evolved out of the lower, and act through and by means of them; destruction of these latter centres prevents any outward manifestation occurring when the higher centre energises. Hence destruction of this lower centre produces complete aphasia. Speech cannot ensue from energising of the lower centre, because that centre is destroyed; neither can it ensue from energising of the higher centre, for though that centre may be itself intact, the channel through which its activity was manifested is destroyed. But it is to be remembered that in energising of this higher centre there are two series of phenomena. It causes movements of the articulatory muscles by a physical process, and simultaneously with these movements of the muscles ideas of words arise in the consciousness. Hence, since in these cases (of ataxic aphasia), the centre does discharge, although that discharge produces no outward manifestation and the patients are deprived of outward speech, they yet retain those intellectual processes in which words serve, and hence show little or no mental defect. From the nature of the injury (blocking of the middle cerebral artery) to which aphasia is usually due, it must commonly happen that the lower centre cannot be damaged without the higher one suffer-

ing in an equal degree. In these cases the patients still remain able to understand what is said to them; and this, the most automatic of all the uses of words, is believed by Dr. Hughlings Jackson to be chiefly due to the action of the opposite hemisphere.

Suppose, however, the lesion be in a higher and more complex centre. In this case the simpler articulatory movements, which originate in centres below (nearer to the muscles than) the injured part, remain unimpaired; but the abrogation of the function of the injured centre produces a double effect. First, on the physical side there is loss of those special movements, resulting in spoken words, which were produced by the discharge of the injured centre; second, on the physical side there is loss of the ability to revive in the consciousness those words whose anatomical substratum has been lost. There is a true loss of memory for words. It is particularly insisted that this also is on the physical side a *motor* defect. It is true there is no paralysis. The muscles concerned can be moved freely and strongly, and in various combinations; but they cannot be used in those combinations which were represented in the injured part, or which ensued when the injured part discharged or energised. What is lost is not the ability to move a certain muscle, but to perform a certain movement.

Great as is the philosophical interest attaching to this question it must not be forgotten that it has an immediate practical value of considerable moment. If the nervous centres concerned in speech exhibit a continuous series increasing in complexity, speciality, and voluntariness from the lowest to the highest, it follows that defects arising from lesions at different levels in the series must correspondingly exhibit all degrees of these qualities. As a matter of fact it is found, and it will be admitted by all observers, that no definite boundary can be drawn between cases of ataxic and cases of amnesic aphasia, or cases of "loss of the power of co-ordinating the muscles of articulation" and cases of "loss of memory for words." It is true that cases do occur exhibiting the symptoms of one form or the other with marked distinctness, but it is also true that between these extremes there is every possible intermediate degree; and not only so, but the same case may at different periods of its history pass through every degree from actual inability to say more than yes and no, to an elaborate amnesic aphasia, and finally to complete recovery.

A second consequence of this hypothesis is that there will be a certain order in the symptoms. Dr. Hughlings Jackson believes, and his reason for so believing will be apparent from what has gone before, that in this as in all other lesions of

nerve centres "the most voluntary or most special movements, faculties, &c., suffer first and most" and recover latest, while the most automatic suffer last and least and recover first. In an extreme case of aphasia the patient has lost speech altogether except the simplest and most general of all propositions, yes and no, or he may also retain the ability to utter oaths—another very automatic form of expression. And even of these words he may have lost the voluntary use, he may be unable to utter them at will, although he does so automatically when provoked. Then in slight cases of aphasia the patient may have a large vocabulary, may be able to utter numerous words, but the most voluntary and special use of words, that of collocating them in propositions, he has either lost or retains only imperfectly and as regards the simplest of propositions. Other corollaries of this hypothesis, which fit in with and explain the phenomena of mistakes in words, "stock utterances" and other conditions associated with aphasia need be only referred to here. We have been thus particular in expounding these peculiar views because they are so generally misunderstood. Dr. Hammond, for instance, says (p. 204):—

"It is very evident, however, that the view of Dr. Hughlings Jackson thus enunciated and indorsed by Dr. Ferrier is not of uniform application to the phenomena of aphasia, for, though in some cases there is no loss of memory for words, but merely a loss of the voluntary power to utter them, there are many other cases in which the patient has lost the memory of words and has not lost the voluntary power to articulate them. Thus the person who speaks of his boots as his 'topsails' has not lost the voluntary power of saying 'boots,' for he can utter the word when asked to do so—an act which would clearly be impossible if he had no voluntary power to say 'boots.'"

Now, in a previous page Dr. Hammond has quoted from Prof. Ferrier: "What is lost in aphasia is, as Dr. Hughlings Jackson so clearly discriminates, merely the power of voluntarily using words *to express ideas*" (italics inserted) which is a very different thing from the power of voluntarily articulating the word. In the instance given by Dr. Hammond, when the patient said "boots" he did not use the word to express an idea. He uttered it parrot-like and without reference to its meaning, and he *had* lost the power of voluntarily using the word to express ideas, for he could not utter it appropriately, that is, when he wanted his boots; in other words, he had lost the voluntary use of the word while he retained its automatic use.

The chapter on cerebral tumour is scanty and imperfect. Due prominence is given to the important symptom of severe pain in the head, but no mention is made of the very important

and very common symptom of severe persistent vomiting. Dr. Hammond does indeed mention that there may be disorders of stomach, bowels, and kidneys, and of respiration and circulation; but he lays no stress upon these symptoms, although undoubtedly next to pain the earliest, commonest, and most prominent if not the most characteristic symptoms of cerebral tumour arise from these disturbances. What the connection is between disease of the brain and serious disturbance of the functions of the viscera, why vomiting, constipation, slowness and other disturbance of the pulse, rapid, slow, unequal or irregular breathing should be so frequently, one might almost say so constantly, associated with certain forms of brain disease is at present quite unexplained, but the connection has long been recognised and verbally fixed in such familiar expressions as cerebral pulse, cerebral breathing, and *tâche cérébrale*.

The section on diseases of the spinal cord gives a very complete account of these affections as developed by the recent researches of the French school. Perhaps no department of medicine has so rapidly advanced within the last few years as that relating to spinal cord disease. This advance is owing chiefly to the labours of Duchenne, Charcot and Vulpian, and their pupils, which are at present far too little known in this country. Dr. Hammond follows mainly the classification of inflammations of the spinal cord which Clément has founded on the descriptions of these observers, and a portion which is here reproduced in order to show how far advanced the French observers are in their knowledge of the diseases of this organ.

The inflammations to which the spinal cord is subject are divided primarily into the acute and chronic, the latter process being identical with sclerosis.

Taking as an illustration the parenchymatous forms of chronic myelitis, they are first distinguished into those which are primary and those which are secondary to lesions above or below the part diseased, and each of these forms, primary and secondary, is further divided according as it affects white or grey matter. The chronic primary myelitis which affects the white matter occurs, according to the account furnished by these observers, in strictly localised regions corresponding with the physiological functions of the cord. Thus it may affect only the median part of the posterior columns, producing a train of symptoms peculiar to sclerosis of the bands of Goll; or it may affect the external part of the posterior columns, causing locomotor ataxy. Or it may single out the lateral columns, giving rise to the symptoms of symmetrical lateral sclerosis, or it may in addition to the last involve the anterior columns, occasioning the addition of muscular atrophy to the symptoms of the last malady.

The chronic primary myelitis which affects the anterior horns of the grey matter causes progressive muscular atrophy, and when it extends higher to the nuclei of the medulla produces glosso-labio-laryngeal paralysis. This summary of a portion of the subject will suffice to show the extent of the researches mentioned, and however surprising it may appear that so chronic and degeneration-like a process should exhibit such strict and peculiar localisation, it must still be admitted that the French observers claim no more than their recorded observations will support, and that up to the present time a good deal of corroborative testimony has been placed on record and little or none of an antagonistic nature. On the other hand, there are certain forms of spinal cord disease, presumably sclerotic, which find no place in their category, and other diseases such as tetanus, which are included by these in cord inflammations may yet be found to belong to the pathology of other organs. There can be little doubt, however, that Duchenne, Charcot, and Vulpian have indicated the main outline of the subject, and what remains to subsequent observers is but the filling-in of details and the rectification of the frontier. The various affections are treated of by Dr. Hammond in a very complete and interesting manner, the chapters on progressive muscular atrophy, pseudo-hypertrophic paralysis and glosso-labio-laryngeal paralysis being especially worthy of notice, as is also that on locomotor ataxy.

Under the head of spinal anæmia or spinal irritation, Dr. Hammond describes an extraordinary aggregation of symptoms which includes in various combinations almost every manifestation of disease to which the nervous system is liable. Postponing for the moment a consideration of the disease, we must first enter a strenuous protest against the abuse of the term "irritation," which is here used in a sense perhaps the least justifiable in which even that long-suffering word has been applied. If "irritation" mean anything (which is not always the case) it surely means in its active sense the application of an irritant; in its passive sense the condition of suffering from such application. This is a mere question of grammar. In physiology any influence which increases the normal function of a part is called a stimulus, and any influence which, differing from a stimulus in kind, intensity or length of application, not only increases but alters the function so as to make it differ from the normal, is called an irritant. Now, as far as appears, it is not pretended that in "spinal irritation" there is any direct or indirect application of an irritant to the spinal cord. The pathological condition is said to be one of anæmia, and it may be laid down categorically that there is no instance in the whole

range of pathology, of persistent overaction being due to anæmia. It would be a violation of physical as well as of physiological law. It is probable that what is meant is spinal irritability, that is, a condition of the cord in which the functional power is not increased or may even be diminished, but in which a less than usual stimulus is needed to produce a given response, and a readier, a stronger or an altered response takes place to an ordinary stimulus. But indeed he would be very foolhardy who would venture to attach a definite meaning to the word irritation as it is ordinarily used. Dr. Hammond (p. 400) speaks of headache, irritability of mind, vomiting, pain in the stomach, palpitation, tremor, and "an erethism of the generative system" as "irritations" due to anæmia. Here are mental states and physical states, states of overaction and states of underaction, active states and passive states all jumbled up together under the heading of one slipshod indefinite term.

Passing on now to a consideration of the disease itself, we find that there are said to be two forms of spinal anæmia, one affecting the posterior columns, the other the anterolateral columns. It is to the first form that "spinal irritation" is attributed. Passing over the great inherent improbability of a persistent simple anæmia enduring for years and limited to a small portion of an organ, we have next to consider the symptoms, and here it may be said at once, that a large proportion of them are not cord symptoms at all. The only indication of their having a spinal origin is the coexistence with them of tenderness over the spine, which is called by Dr. Hammond "the essential symptom of spinal irritation;" but which is an exceedingly common symptom in all states of debility, irrespective of cause. Vertigo is due to an affection of either the ocular muscles or the internal ear; headache, fulness, and a sense of constriction across the forehead, are such very general symptoms that they may be disregarded; noises in the ears may be due to a variety of causes, only one of them—altered vascular pressure—having any probable connection with the spinal cord. Nausea, vomiting, pyrosis, flatulence, and acidity cannot be admitted as spinal cord symptoms unless it is clearly shown that there is no disease of stomach, and unless all the recognised causes of dyspepsia can be certainly eliminated; a most difficult condition to fulfil. Indeed, to most minds little less than experimental demonstration would be required to show that these dyspeptic symptoms could depend on anæmia of the cord. The attribution of chorea and epilepsy to anæmia of the posterior columns of the cord is a diagnosis which, having regard to the present state of our knowledge of the pathology of these diseases, cannot be allowed; and when acute mania is added to the

symptoms we can only wonder that a physician of Dr. Hammond's clinical and pathological attainments could have made a statement so extraordinary.

V.—Orthopædic Surgery.¹

THE surgery of spinal curvature, rachitic joints, club-foot, paralysis, and similar chronic and evident deformities, has a great tendency to pass out of the hands of the medical practitioner and care of the pure surgeon into those of so-called specialists in these departments.

The tediousness and wearisome sameness in the symptoms, and especially the peculiar feelings of regret and even of disgust which such deformities inspire in the hearts of friends and relations, tend to induce in the minds of these relations an irritability and dissatisfaction with their own doctors who seem powerless to prevent, and with the surgeons who seem so slow in curing such a sad deformity. Hence a longing for novelty, pursuit at all hazards of new and fashionable specialists, and a readiness to believe any promises of cure however hyperbolic, to try any nostrum and wear any apparatus, however preposterous.

The chronic nature and slow progress either for good or evil made by this class of cases renders quackery possible, even easy, and indeed puts a premium on imposture. For example, a growing girl from long sitting at her books or music has got an ugly-looking, though really insignificant curvature of her dorsal spine. She goes to a surgeon who knows his work and is a gentleman; he sees what is coming, he recommends cessation of the work—rest on her back for part of the day, a gallop on a pony in seaside air for the rest of it. He may give cod-liver oil if needed. He gets his two guineas and says good morning. The child will recover, nature will get the credit, the surgeon's visit will soon be forgotten by patient and friends. But an officious friend may say, Go and see Mr. Blank; he is so clever at spines; he does nothing else; he will cure her.

¹ 1. *Lectures on Orthopædic Surgery and Diseases of the Joints, delivered at the Bellevue Hospital Medical College, Winter Session of 1874-5.* By LEWIS A. SAYRE, M.D. 274 wood engravings. London.

2. *Lectures on Orthopædic Surgery delivered at St. George's Hospital.* By BERNARD E. BRODHURST, F.R.C.S. Second edition. London.

3. *The Cause of the Rotation in Lateral Curvature of the Spine.* By A. B. JUDSON, A.M., M.D., Lecturer on Orthopædic Surgery to the Woman's Medical College, New York, &c. New York.

4. *Nature and Treatment of Deformities of the Human Body.* By LAMBERT H. ORMSBY. Dublin, 1875.

5. *The Gentle Treatment of Spinal Curvature.* By H. H. BIGG. London.

They go. Mr. Blank, after many measurements and much manipulation, shakes his head gravely; calls the father aside: Ah sir, be thankful you came so soon; a most formidable case this is—would soon have been hopeless. You must have an apparatus for your child; she must come into town and take lodgings near me. I must see her daily for a month or two, and there is one man, a most accomplished mechanic—he and I will devise an apparatus; we cure these diseases in the most gentle and mild manner without suffering. The child goes to the eminent mechanic; is provided with apparatus, a little expensive perhaps, but beautifully finished and *so mild* in its action. Mr. Blank calls daily and with extreme gentleness shifts a little key which by a rack-and-pinion adjustment presses in the most gentle manner on the displaced bones and reduces the deformity. The child is kept on her back, perhaps in bed. She is fortunate if she escapes with weakened general health and a constipated habit. The father is lucky if he gets out of London with a residuum in his pocket of £500 spent on Mr. Blank's 250 visits, the mechanic's apparatus, and the apothecary's bill. He must have something to show or to tell of for his money when he goes home, so the natives will hear of Mr. Blank's gentleness and great practice, of the mechanic's great ingenuity, and will for themselves discover what an attentive father the patient has.

Such specialists can survive and make a living only in large towns in which there is much wealth and many patients, or at least to which patients are attracted; but if they so far limit the range of their practice and consequently the area of possible patients, they must have some means, legitimate or otherwise, by which to bring their names and addresses prominently before the public and thus to attract patients.

Non cuivis contigit adire Corintho, we cannot all start special hospitals or even become surgeons to the Orthopædic, so in default of these advantages we must write a book, either on the whole range of our subject, or, what is even better, upon some small and lucrative corner of it. This book may be for the information of one of two parties—either the profession or the public. If written in an honest spirit, out of the fulness of the knowledge which we have either really gained or at least think we have gained, the book may be useful, and lay the foundation of a true and honorable distinction. It may make the profession feel that we have done work in the subject or know more about it than they do, and they will bring their patients for consultation to mutual advantage and satisfaction. But on the other hand the work may be addressed primarily to the public. It will be loudly written and flashily illustrated;

it will have a fascinating title such as "Cure of curvature of the Pons Varolii without the use of the Knife (with a capital K) or confinement to bed." It will lie on instrument makers' counters. It will be steadily advertised in the first page of the weekly journals, and not seldom in the 'Times.' The public will in the end, if the capital for advertisements and for the printers' bill fail not, flock to the house, but as a rule they will come without their doctors. Perhaps they have not told these doctors they were going.

Such patients will pay,—some of them largely—the surgeon may make a large fortune in a few years; but his reputation and his practice are built on sand—only while he is the fashion, only till some other similarly disposed, can talk and write more loudly and advertise more successfully, will the harvest last, and after all his guineas and all his editions of his work his name will be forgotten and his so-called discoveries only mentioned to be contradicted.

Great surgeons and teachers have, by force of circumstances, special liking, special opportunities, ended by being great specialists. But they were great surgeons first, well educated, open-minded, honest, hardworking men who made the great discovery which has immortalised their name. Stromeyer and Dieffenbach were great surgeons first, orthopædic surgeons in the end. Curling and Thompson have each only limited their range of practice by necessity.

In so-called orthopædic surgery there is no reason whatever for its being made a so-called specialism; indeed, many reasons *against* any such isolation. To diagnose and treat properly individual cases of spinal curvature, for example, requires the highest surgical and medical acumen combined, for the varying relations of bone and ligament, heart and lungs, must be carefully noted.

To arrange a boot for clubfoot without division of tendons needs only an intelligent shoemaker and nurse. To cut the tendons is one of the easiest of surgical operations to any surgeon who knows his anatomy, and a Fergusson or a Pollock or a Syme would cut tendons better than the most exclusive specialist in that line. Indeed, we are rather inclined to believe that a race of skilled mechanics who would *do as they were told* both in the manufacture and fitting of instruments, who would not ape the medical professor or imitate his charges, would be a most valuable addition to our means for treating deformities.

The different works the names of which head this brief notice vary much in their character and object. We say object, because even when the object is not stated it is fair to infer it from the method of the performance.

Professor Sayre's work is in our opinion one of extreme in-

terest and value. Containing many propositions which will startle, some views in the promulgation of which he stands almost alone, it is full of sound sense, original observation, and a manly personality very characteristic of the man himself. A happy vein of almost unconscious egotism and what may appear to us phlegmatic Saxons as a chirpy self-conceit, or self-satisfaction, is not a real deformity in the work.

In his clinical lectures there are some amusing yet still very instructive and striking bits of dramatic art, one or two of them almost hyperbolical or quasi-miraculous in the immediate effects produced by the surgical manœuvre.

One specially strikes us as nearly comic in its details; it is recorded at page 287 et seq. We have the picture of a little girl sadly emaciated and exhausted, fixed by her agony from diseased hip-joint in a most uncomfortable position; her thigh flexed and adducted, and with sinuses leading to necrosed bone. The operation of excision is described, the wound is dressed with oakum, the position of the limb is rectified, and the patient put up firmly in a pair of wire breeches and propped against the wall. The whole business has taken half an hour, and then we see her in another picture propped up against the wall and smiling graciously. It reminds us of a little tale of our early surgical days soon after the introduction of anæsthetics. A gentleman walked into the consulting room of a friend, showed him a bad elbow; in a moment he was laid on the table, chloroformed, and in half an hour he left the house with his joint excised, his arm in a sling covered by his cloak, and with his pipe lighted for him by the sympathising and exultant surgeon safe in his cheek.

But apart from such little peculiarities, personal and ethnic, the work is a good one, owing nothing to fine writing, nothing to revision or care. The author tells us it was written down from his oral lectures, some of which were unprepared, as being clinical on cases seen for the first time.

The author's aim has been to tell what he thinks is true in such a way as not to be misunderstood; in this he has succeeded to a marvel.

The first lecture gives a brief historical account of orthopedy, as Professor Sayre spells it, and contains quotations and dates, and list of names; indeed, is obviously an introductory lecture of the old school. Two quotations from it are characteristic.

"Pretenders and quacks invariably publish accounts of their wonderful cures, and the miracles they have performed, never laying down any laws or rules to aid another in performing the same cure in similar cases. And this, gentlemen, constitutes one of the essential differences between an honorable physician and the quack. The

one labours to disseminate and diffuse his knowledge for the benefit of his whole profession, in order that he may relieve as much of human suffering as is within his power, the other endeavours to conceal the little knowledge he may possess for his own particular profit or gain (pp. 2, 3).

"If, therefore, I shall at any time lay down any doctrine the truth of which I cannot practically demonstrate and establish by bringing before you genuine cases to illustrate it, you are at perfect liberty to discard such teaching.

"Never be governed by the *ipse dixit* of any man unless the demonstration accompanying it, or your own careful investigation, shall convince you that the principles enunciated are true. If, by means of clinical cases, I shall succeed in clearly substantiating the doctrines I shall teach, please endeavour to learn the precise method of management adopted in each case, so that whatever success I may secure by treatment you may also obtain" (p. 8).

In Lecture III we have a brief account of a form of reflex muscular contraction occurring especially in children as a result of genital irritation from congenital phymosis and adherent prepuce. Since the relation of such irritations to deformities was first in 1870 pointed out by Professor Sayre, numerous cases recorded in the medical journals have proved the frequency of the malady and the advantage of the treatment.

The general rules which should guide the how, when, and where of tenotomy are well laid down, and with this advantage, that we can learn distinctly and clearly what *Sayre* does and how and why *he* does it, without being wearied and distracted by reading about what everybody else has done since the flood. Thus, Sayre holds about knives:

"These blades are made of various shapes; some straight, and some curved, with the cutting edge either on the convex or concave border. The sharp-pointed tenotomes usually found in the shops should never be used, as they are liable to puncture tissues which should be unmolested; and their use in the neighbourhood of important vessels and nerves is very hazardous" (p. 27).

And about treatment:

"The next important question is, Shall the parts after section of the contracted tissues be restored as nearly as possible to their normal position as once; or shall a delay be made of a few hours, or a few days, until the external wound has permanently closed and the inflammatory action which may follow the operation has subsided? For many years my teaching was to secure the limb in its deformed position until the external wound had closed and the inflammatory action had subsided; but at present it is that the deformed parts should be restored AT ONCE as nearly as possible to their normal position. This is the general rule which I feel willing to

lay down as the one which should govern you in the majority of cases, but to this rule there are notable exceptions. In all deformities dependent upon normal muscular action alone, whether paralytic or spastic, restore the parts as nearly as possible to their normal position immediately after section of the contracted tissues has been made. In all cases, however, of acquired deformity, which depends upon previous disease of a joint, terminating in fibrous ankylosis, and in which section of the contracted parts becomes necessary, the division should be made, and the external wound be permitted to heal before resorting to force for the purpose of breaking up the ankylosis. If motion and force are applied in this class of cases *immediately* after section has been made, air may enter the wound, inflammation follow, and suppuration be established" (p. 29).

This may with advantage be compared and contrasted with Brodhurst's opinion on this point.

His lectures on talipes are good, containing many original devices; indeed, many practical surgeons will be pleased with the little hints as to the application of plaster, modelling of splints, making of boots, &c. Patience and trouble seem the chief factors in his success.

Corns, bunions, flat-foot, genu valgum, are carefully described; and a righteous attempt is made to persuade surgeons that it is their duty, as it certainly will be for their profit, to attend to these despised but by no means despicable ailments.

Surgeons will turn with even greater interest to Dr. Sayre's views as to the pathology and treatment of diseases of the joints. The ankle-joint is the first on the list, and in his account of the diseases to which it is liable he ventilates his peculiar views as follows:

"In a great majority of instances what we have to deal with is an extravasation of blood beneath the synovial membrane, or between the cartilage or bone, quite analogous to the 'blood-blister,' which is formed upon the external surface whenever the skin is severely pinched but not broken. This may occur either upon the astragalus, or at the lower extremity of the tibia, or, still more commonly, as the result of pressure produced by the astragalus against the inner surfaces of the malleoli, which are not sufficiently protected to resist severe pressure. Under such circumstances, no swelling occurs that can be seen; there is pain, probably, but the cases are very liable to be neglected, their importance overlooked, and thus a slight injury, producing only a trifling damage at first, may be permitted to go on and develop the most serious conditions, ending in inflammation, which goes on to softening of the bone, necrosis of cartilages, and destruction of all the tissues involved in the joint. The inflammation may extend to other bones, and you may have as a result softening and caries of all the bones of the tarsus, as

in the case you now see before you (see fig. 105). These are the cases that are called 'scrofulous disease of the ankle-joint.' There is no scrofula about it in the vast majority of cases. It is simply inflammatory softening, ending in caries and necrosis of the bones, and ulcerative destruction of other tissues entering into the formation of the joint; and instead of being constitutional in its origin, dependent upon a constitutional cachexia, it is simply inflammation of the joint dependent upon injury, consequently *traumatic* in its origin" (p. 155).

This pathological standpoint is full of most important results, both as to symptoms and their explanation, and still more as to treatment.

Dr. Sayre's treatment is based on the above distinct theory, and hence is guided by fixed principles. It may be summed up under three great heads: 1. Rest; 2, relief of tension; and 3, attention to the general health, and eventually to the motions of the limb. We cannot quote at length; and would spoil by abstracting his account of the apparatus he uses to effect his purpose of the different parts, but must refer the reader to the work itself. One little bit of practical empiricism we may note, *i.e.* that for many years Dr. Sayre has refused to operate by exsection of the ankle- and wrist-joints, but substitutes for the operation the following treatment:—Cut into all the sinuses, and remove sub-periosteally all the dead or dying bone in the neighbourhood, saving most carefully the periosteum. Then draw through the sinuses thus opened a large seton of oakum, saturated with Peruvian balsam, the ends of which are to extend beyond the ulcers for several inches. The limb is then to be fixed into the foot in a comfortable position by a plaster-of-Paris splint, and plenty of room left for dressing the sores, which are to be compressed carefully by pads of oakum. The setons are to be moved daily.

Capital cases are recorded as illustrating the results of his treatment in the difficult joints.

The hip-joint is treated most fully, and with a master hand. Practical surgeons will read with interest the carefully compiled table of the results of fifty-nine cases of excision of the head of the femur for hip disease, of which the following is an abstract.

"Synopsis of Fifty-nine Cases of Exsection of the Hip-joint for Morbus Coxarius."

"Thirty-nine of these cases are now alive. Of these, twenty recovered with motion and less than one inch shortening; eight recovered with motion and more than one inch shortening; two recovered with ankylosis, and nine are still under treatment, with every prospect of good results.

"Twenty of these cases are now dead. The cause of death in each case, as well as the length of time after the operation, is as follows :

"Case 2 died from exhaustion on the eighth day.

"Case 3 died from exhaustion in two months.

"Case 10 died from tetanus on the fifteenth day.

"Case 14 died from exhaustion in two weeks.

"Case 15 died from double pneumonia on fourteenth day.

"Case 16 died from dysentery seventeen months after the operation, wounds having been almost closed for some months.

"Case 19 died from marasmus two years and two months after the operation. Wound had been closed for six months.

"Case 20 died from dysentery in two weeks.

"Case 23 died from dysentery eight months after the operation.

"Case 27 died from phthisis two years after the operation. Wound healed for more than a year.

"Case 29 died from sunstroke from exposure on the fortieth day.

"Case 30 died from exhaustion in three weeks.

"Case 32 died from fatty degeneration of the liver and kidneys twenty-three months after the operation. Wound nearly healed; three inches of new bone formed.

"Case 36 died from exhaustion in eighteen months from progressive disease of the ilium.

"Case 39 died from amyloid degeneration of kidneys in three weeks.

"Case 42 died from sudden suppurative nephritis two years and three months after the operation. For nearly a year had been able to walk without support.

"Case 43 died from dysentery thirteen months after the operation. Wound had been entirely closed for four months.

"Case 44 died from nephritis from cold nineteen months after the operation. Had been well for nearly a year.

"Case 47 died from exhaustion two and a half months after the operation.

"Case 49 died from exhaustion in one month.

"Of these twenty cases that died, eight had recovered from the operation some time previous to death; which was caused in each case by some other disease entirely foreign to the operation (Cases 16, 19, 27, 32, 36, 42, 43, 44).

"Of the twelve remaining, four died from acute intercurrent diseases, such as tetanus, double pneumonia, dysentery, and sunstroke (Cases 10, 15, 23, 29). This leaves but eight who have died from the exhausting effects of hip disease, without some intercurrent complication (Cases 2, 3, 14, 20, 30, 39, 47, 49)," pp. 326, 327.

Even those who may differ from Dr. Sayre in his pathology or treatment must be impressed with the ingenuity of his theories and his patience, resources, and successful treatment.

Pott's disease of the vertebræ meets with technical skill

active and original. Suspending the patient in an ingeniously contrived sling or support, he surrounds him with a well-fitting plaster-of-Paris jacket, which seems to relieve pain and pressure, and allow erect position, and yet, when properly put on, not to interfere with respiration.

The treatment of ankylosis by myotomy, tenotomy, and the powerful, sudden, and repeated movements called *brisement forcè*, is fully described and illustrated by cases; and the concluding chapter of the book contains some interesting information about less known and rarer deformities. A series of cases of wrist-drop and other symptoms of lead poisoning, from the use of 'Laird's Bloom of Youth' as a cosmetic, are very instructive.

We could easily, did space permit, have quoted more and criticised at greater length; but we would rather recommend our surgical readers to buy and read the book.

There are trifling defects and blemishes, perhaps inseparable from the strong individuality of the author, or from the mode of preparation and publication, but on the whole the work is a sound, sensible, original, and suggestive work.

Mr. Brodhurst's work having reached its second edition, and consisting, as it does, of the pith and marrow of a course of lectures frequently delivered, has both concentration and arrangement.

In the first chapter we have an attempt made to arrange and formulate the causes of congenital and non-congenital deformities, the most frequent being disordered nerve influence, rickets, and debility. Duchenne's paralysis is briefly alluded to, but the account of it is very meagre, no attempt being made to describe or explain the pathological anatomy of the disease.

An interesting section in Chapter III relates the history of the operation for division of tendons. Mr. Brodhurst starts with Delpech's rules, which he says are admirable, and still recognised as authoritative. They are—

1st. The tendon to be divided should not be exposed, but its section should be made by inserting the knife at a distance from the tendon, and not by making an incision in the skin parallel to it. There is danger of exfoliation of the tendon unless this precaution be taken.

2nd. Immediately after division of the tendon the divided extremities of the tendon should be brought into contact, and so held by a suitable apparatus till reunion is accomplished.

3rd. As reunion can only take place by an intermediate fibrous substance (*organization inodulaire*), gradual and careful extension should be made to give the required length to the shortened muscle before solidification takes place.

4th. Extension being complete, the limb should be fixed in

this position, and be so retained until the new substance has acquired that firmness of which it is susceptible (pp. 41, 42).

He then goes on to detail some experiments of his own, made by dividing the tendons of animals, on a quite different plan from those made by previous experimenters. In his experiments on the Achilles tendon of twenty rabbits (instead of, as had been done by others, allowing the animal to move about after the section) he kept the limb at rest, thus making the conditions of the experiments resemble those in the human patient. The experiments and their results are given in some detail, but they were done in three series, to show—

1st. That when a tendon has been divided it may unite without leaving a permanent cicatrix.

2nd. That the new material may be extended to any required length. When, however, the divided ends of the tendon are widely separated and kept apart reunion will not take place.

3rd. That the new material does not impair the power of the muscle, except when it is so elongated as to prevent entirely or in part the action of the muscle.

From the above it will easily be seen that the principle on which Mr. Brodhurst treats his cases is, first, to divide the antagonising tendon, if necessary; then to put on an apparatus for remedying the deformity so soon as the puncture is healed, and then gradually to extend, for a period of about six weeks, till complete restoration to shape is attained.

On the one hand, if too slow extension be made the distortion will not be removed; while, on the other, if it be done too rapidly the uniting medium of the tendon may be destroyed, and the natural action of the muscle then be lost entirely.

The different varieties of talipes being well described and figured, the student should have little difficulty in understanding the account of this deformity.

The chapter on "False Ankylosis" is interesting. Mr. Brodhurst's own experience has been very extensive. He says—

"It was only when tenotomy and anæsthesia were combined that the operation of forcible flexion could be looked upon as free from danger. I have performed upwards of 800 operations of this nature, and have never known any *contretemps* whatever—neither fracture nor dislocation, nor pæmia, nor inflammation."

To obtain such results, however, the operation must be performed *secundum artem*, rigid tendons to be first divided, and then the punctures allowed to heal; then under chloroform fully administered, and with the limb to be operated on firmly fixed, the adhesions are to be instantly ruptured by force

applied in the direction of flexion. Careful bandaging and splints follow, to ensure rest for some time.

Several cases of great interest are detailed, in which success had been obtained under apparently most adverse circumstances.

In the chapter on "True Ankylosis" an account of the treating of the operation of subcutaneous osteotomy is given, and the author's claims to priority at least implied.

A very rare and remarkable case is described in which Mr. Brodhurst reduced, or supposed he reduced, a dislocation of the humerus, 175 days after its occurrence. After free movements of the head to and fro and in rotation, an upward movement of wrist and arm made the head of bone go *in* (?) with a slight click (p. 174).

For contraction of cicatrices of burns, &c., gradual extension by mechanical means is highly praised, and seems to have in many cases produced excellent results; in severe ones the previous division of tendons and fasciæ may be required.

There are some interesting and well-illustrated chapters on the pathology and treatment of curvature of the spine in different directions, with elaborate views as to the causation of the different curves and twists. Put in the language of ordinary life, we fear the student will not make much more out of it all than this,—“Weakness of constitution, awkward habits and tight stays, may induce weakness of muscles of back, from which all sorts of deformities may result if you do not take care. Treat these by exercise, and avoiding weariness and bad habit, and to appear to do something, put on an apparatus which will very slowly ‘*unfold*’ the curve.”

Angular curvature with caries of the bodies of the vertebræ receives a shorter notice. We would have desired a more special account of atlo-axoid disease, and the value of the cautery in its early stages.

Rest, counter-irritation, and support, *especially rest*, are the chief points to be attended to in the treatment of this terrible malady in whatever part of the spine it begins.

The proper management of the resulting abscesses is perhaps not specially within the domain of orthopædics, else we might have had an opinion on antiseptics.

On the whole it is a good useful book, intended perhaps by a teacher as a text-book for his pupils, but at least honestly addressed to the profession and not to the public.

Very different in its character from either of the works we have described is the brochure entitled with a pretty egotism, ‘*The Gentle Treatment of Spinal Curvature*,’ as if this and this only deserved the name of *gentle*. It is little else than a ‘gentle’ advertisement of the wares of a pushing tradesman and

mechanician. Directed in its aim to catch the public, exhibiting ignorance of pathology, with boldness of assertion, it possesses no claim to notice in a professional review. Its most objectionable feature is the apparent purpose to encourage the resort of patients to the author's establishment, without their doctor, with the object of consulting him as a sort of surgical authority.

Dr. Judson's pamphlet on the 'cause of rotation in lateral curvature of the spine is a good example of the kind of work absolutely free from any suspicion of advertising. It is addressed to the profession only, and to those of the profession who have time to think, and are capable of following pretty stiff reasoning. It is concisely written, free of all egotism, and implies in its author not only a large amount of previous consultations of authorities, but also the power of original observation and scientific reasoning.

He first describes the cases in which rotation of the vertebræ on each other takes place in lateral curvature; then points out that there are certain cases in which this rotation is absent; then he shows that the degree of rotation is in proportion to the degree of curvature.

The cause of the rotation is a vexed question. Herman Meyer discovered that the anterior portion of the spinal column possessed great expansibility, and the posterior portion great compressibility, and inferred that, when the lateral curvature took place, the posterior portion of the column would seek the concavity, and the anterior portion the convexity of the curve. Shaw found in structural changes in the articulating processes the explanation of the occurrence of rotation. Bampfield, Dick, Bigelow, and Barwell, each have different explanations, physical, mechanical, and muscular, and are at variance as to whether the rotation is a primary movement of lateral curvature, or a mere sequel or incident.

The author's explanation is as follows :

"The rotation of the vertebræ in lateral curvature of the spine consists in an unequal lateral displacement of the body, and the spinous process; the former being free from lateral attachments, and therefore departing from the median plane, while the latter is held in the median plane by its muscular and fibrous attachments."

This opinion is supported by an ingenious view of the attachments and origin of the various muscles of the back and limbs, in which the author shows how powerfully most of these act in holding the posterior segment of the vertebral column, and specially the spinous processes in the median plane. In the

same manner he proves that the ligaments, fasciæ, and aponeurosis have all the same tendency. He then shows how little there is of either muscular or ligamentous structure to keep the anterior portion of the vertebral column in the median plane. He then illustrates by simple diagrams the possible axis on which rotation may take place in a given body. These are a central axis, a peripheral axis, and a remote axis. He believes that the rotation of vertebræ in lateral curvature is rotation on a remote axis.

By an ingenious preparation of the bones of the spine fitted on elastic bands and provided with mobile check ligaments, he illustrates the production of lateral single and double curvature.

In the latter pages he contrasts the explanation of curvature and rotation on this theory with the other explanations, much to the advantage of his own. Those who will take the trouble to study this well-reasoned, concise, and modest little pamphlet will be impressed with the force of the author's views.

On the principles we have laid down as the genesis of works on orthopædics, the last work on our list will be found a failure. As far as real permanent reputation is sought in it, we fear it will not be found. A mere compilation, with poor arrangement, absurd diction, and many inaccuracies, it may have served its purpose as originally intended, to give students a sort of bird's-eye view of the whole subject of orthopædics in a short course of twelve lectures. The exaggerated style, high-flown similes, and poetical quotations, may suit the juvenile Irish mind, but will not deeply impress any one over the age of seventeen on this side of the Irish Channel.

It must be said in its favour that it seems an innocent little work, and not addressed to the public, but quite honestly to the junior members of the profession.

We cannot say to them that they will get all they want about orthopædics in its simple yet grandiloquent pages; but we may hope that the author's attention having been drawn to the subject of deformities, so many cases may come under his hands that he may be enabled to do some real original work and advance the sum of professional knowledge in a future edition.

VI.—On the Histology of Tubercle.¹

It is from the time of Laennec that the true history of the pathology of tubercle commences. Up to this date the knowledge of pathological anatomy, and, more especially, of pathological physiology was so undeveloped, and founded upon such a false appreciation of the animal economy as to tend to mislead rather than act as a guide. Since Laennec's time, however, the rapid strides that pathology has taken in defining the first principles of tissue changes, have placed morbid histogenesis on quite a new basis, not one of mere theory, but consisting in great part in an accumulation of accurate facts. However far we may yet be removed from a definite knowledge of *ultimate causes*, we are certainly on the right path to their elucidation when we accept nothing more than we can demonstrate, and trust to no theory which we cannot experimentally verify.

Although Laennec's ideas regarding tubercle may seem strange to the modern pathologist, yet his writings will always be of great value from his wonderfully correct observation of facts, and his not having advanced more theory than was compatible with what he had himself noticed. Had he possessed the same means of investigation which we at present have, his speculation on the nature of tubercle, generally, would, no doubt, have been much more guarded. After his time a host of observers arose from whose combined efforts a certain amount of advance was made in what may be called the tubercular and tubercle-like affections, but, in reality, little or no material change was made in the doctrines of Laennec until within the last few years. It is only lately that we have begun to differentiate between what is tubercular and what is not tubercular, and to define with something like accuracy what we mean when we employ the term "tubercle." Tubercle, no doubt, was a name which was formerly employed to express any nodular enlargement in an organ or on a surface; and, as nodules of a peculiar character were

¹ 1. E. WAGNER.—*Das Tuberkelähnliche Lymphadenom (der Cytogene oder Reticulirte Tuberkel)*. Leipzig, 1871.

2. Dr. OSKAR SCHÜPPEL.—*Untersuchungen über Lymphdrüsen Tuberculose sowie über die damit verwandten Drüsenkrankheiten*. Tübingen, 1871.

3. Dr. THEODOR HERING.—*Histologische und Experimentelle Studien über die Tuberculose*. Berlin, 1873.

4. Dr. LUDWIG BUHL.—*Lungenentzündung Tuberculose und Schwindsucht*. München, 1873.

5. PROF. W. BRODOWSKI.—*Ueber den Ursprung sogenannter Riesenzellen und über Tuberkeln im Allgemeinen*. 'Virchow's Archiv,' lxiii, 113, 1875.

6. L. THAON.—*Récherches sur l'anatomie pathologique de la tuberculose*. Paris, 1873.

7. E. ZEIGLER.—*Untersuchungen über Pathologische Bindegewebs und Gefäßneubildung*. Würzburg, 1876.

found with extreme frequency in the lungs and other organs, the term, gradually, by a process of specialisation, came to refer to a particular kind of nodule occurring in a certain habit of body, and producing symptoms of a peculiar nature. The term "cancer" no doubt has undergone a similar narrowing of meaning by the same process of specialisation. Laennec sought to limit the term "tubercle" to what he regarded as one particular morbid product. Virchow, on the other hand, endeavoured to show that this same product was not always similar in its composition, and that its mode of formation was widely different in what were supposed to be identical structures. And now, in later times, it is endeavoured to be shown that we can form no correct idea as to the tubercular nature of a part until we have made an elaborate examination both macroscopically and microscopically, and found a certain definite structure. Cancerous tumours formerly included numbers of other new formations totally distinct in their nature, as, for instance, the sarcomata, while the epitheliomata were considered to belong to a different category. It has only been by the accurate labours of such men as Waldeyer and others that we have arrived at a true definition of the term "cancer" founded upon its structural arrangement and its mode of origin. The same holds true of "tubercle," and we may rejoice in the conviction that we have now arrived at, or are approaching, a time when we shall have as accurate a definition of the latter as we at present possess of the former.

From the extensive labours of almost all the modern pathologists who have made the subject of tubercle a special study within the last few years, it seems evident that the naked eye affords us little or no help in defining what is tubercle and what is not. *The truly tubercular structure is essentially a microscopic object.* It is abundantly shown from the works of Schüppel and Hering, for instance, that many structures, which we might say at first sight are tubercular, prove, on minuter examination, not to be so, and others that we probably would have characterised as mere inflammatory deposits in a scrofulous subject prove to be essentially tubercular in their nature. The rough-and-ready methods which have been usually employed for the purpose of investigating tubercular organs must, therefore, be abandoned, and before we can say whether a deposit in an organ is tubercular or not, a systematic examination of the part, aided by the latest improvements in histological technology, must be gone through.

Perhaps the first accurate description of tubercle which is on record is that given by E. Wagner (loc. cit.). It has been proved by subsequent observations that what he described under the name of tubercle-like lymphadenoma is really the essential structure of true tubercle, and it seems that his definition, even yet, holds good as that which is characteristic of a tubercular new formation. In

all true tubercle in the human subject, when it is fully formed, we find this structure, and where we find such appearances revealed in a tissue we are entitled to say that they are indicative of a tubercular growth. His description of it is as follows when found in lymphatic glands :

“ In recent cases there were found within lymphatic glands one, two, or three round or roundish spots, which were composed of exquisitely typical cytogenous tissue. In the centre of these nodules there was constantly a many-nucleated giant cell, of different size, with numerous processes, and separated from surrounding parts by a concentric ring of connective tissue. This cytogenous tissue is distinguished from that of the glandular substance in that the nuclei and cells within the reticulum are larger, that they are easily isolated, and that, in the neighbourhood, a giant cell was always present. The reticulum itself had also a more cellular structure. The lymphadenomatous nodule was placed in the lymphatic gland-tissue, which last was little or not at all compressed by it.”

This was the first description given of such a structure, and from subsequent inquiry it appears that what Wagner originally described as a lymphatic adenoma is really the structure of true tubercle, not only in lymphatic glands, but also in all other organs.

About the same time as Wagner's observations were published on this subject Schüppel (*loc. cit.*) produced his well-known work on tuberculosis of lymphatic glands, and, as he himself says (p. 7), he was surprised to find that his observations exactly coincided with what Wagner had already described. From long-continued research he is persuaded that tubercle of lymphatic glands takes its origin neither in the normal connective-tissue trabeculæ and septa, nor from newly formed connective tissue of an inflammatory nature. It originates, more especially, in the glandular substance of the cortical nodules and medullary tracts, *and the appearance of a giant cell indicates the commencement of the tubercle.* The formation of tubercle is not connected with a previous inflammation and connective-tissue formation in the gland. Fibrous tissue may, however, be formed round the tubercular nodule, and therefrom result nodules which are caseous in the centre, and are surrounded by fibrous tissue. At the same time he is specially emphatic in saying that tubercle, although formed within the glandular follicles, is not to be mistaken for mere hyperplasia of the same. The tubercular new formation is perfectly distinct, and is sharply marked off from the surrounding glandular tissue. The more special description which he gives of the tubercular structures is exemplified in Case 1 (p. 113). The glands were those of a lad aged seventeen years, which had been extirpated on account of their size. Microscopical examination showed numbers of small nodules at different parts of the follicular tissue. They were made up of a network of fibres containing epithelioid elements

in the meshes, and a large giant cell in the centre. Each mesh contained one cell. The giant cell contained sometimes as many as one hundred nuclei. The larger glands contained more nodules than the smaller, sometimes forming as much as one half of their entire bulk. In reviewing this case, he says that here is an individual with incipient scrofulous affection of the glands of the neck, and each nodule within them represents, in truth, the primary form of tubercle. There is, therefore, a primary form of glandular tubercle which can arise as a locally situated affection. It is specially to be remarked, however, that the tubercle alone is primary, not the irritation which may have excited it. Several other cases of a similar nature are recorded, in which the tubercular new formations were identically of the same structure. It is, therefore, clear that in certain enlargements of the lymphatic glands of a so-called tubercular nature, there is a particular very definite structure which has been described in exactly the same terms by two observers working independently of each other. We may, accordingly, presume that the observations are correct, and that tubercle of lymphatic glands consists of the following arrangement of parts. A large many-nucleated giant-cell in the centre, having enormous numbers of processes which run outwards to form a branching reticulum, or, at any rate, become intimately connected with it. Within the meshes of this reticulum are numerous epithelioid cells, while the trabeculæ of the reticulum terminate externally in a concentric layer of fibrous tissue. It may be noticed that Schüppel finds that the formation of a giant cell is the starting-point of the tubercular new formation. It is also to be remarked that tubercle, according to him, occurs in apparently healthy glands, more especially the bronchial, in cases where there is any local infecting source such as caries of the vertebræ (p. 44). Microscopic examination alone revealed the fact of its abundant formation. He believes that the so often met with caseation of superficially placed glands, certainly, in the greater number of cases, is of tubercular origin, an observation which is very far from coinciding with the so generally accepted opinions of the Virchow school of pathologists. He concludes that glandular tuberculosis is an exceedingly common lesion, not always recognisable with the naked eye, which depends on the development of small nodules or tumours. These nodules have so distinct a structure that he knows of nothing else with which they can be confounded. They are always exclusively situated in the vascular follicles of the gland (p. 78). Tubercle is by no means a mere collection of small round cells, but consists mainly of a small sharply bounded tumour of different, but, frequently, considerable size, as well as a reticulum which approaches very near that of adenoid tissue, and in whose meshes the tubercle cells are imbedded. He distinguishes in the tubercular nodule three kinds of cells: (1) *epi-*

thelioid-cells; (2) *giant-cells*; (3) *lymph-corpuscles*. The giant cells are hardly ever missed, and, when they are absent, it is due to the direction of the section not having included them. The giant cell lies usually in the centre of the nodule, sometimes excentrically. There is generally only one, sometimes as many as three, five, or ten. Epithelioid elements are a constant constituent; they fill up the spaces of the reticulum and close in the giant cells on all sides.

Had this description of tubercle been confined to lymphatic glands alone one might have had some doubt as to its being typical of tubercle occurring elsewhere. In 1873, however, Hering published his work on the histology of tubercle, in which he describes a lesion so similar in various organs as to leave no doubt that it is essentially the same (*loc. cit.*). Here, for instance, is his description of tubercle of the pleura. Round or oval collections of so-called adenoid tissue were found which were closely surrounded by circularly placed bands of connective tissue. The adenoid growth consisted of an irregular network which was interspersed with spindle cells, frequently having two to three processes. The processes of these cells by anastomosing formed the reticulum. In the meshes lay many small round cells, specially numerous at the periphery, and close by them lay also nucleated epithelioid elements. *The nodule generally possessed a differently shaped giant cell* placed in the centre or at one side, and, in some nodules, there were several giant cells having numbers of processes which anastomosed with one another. These giant cells were composed of a differently shaped, sharply contoured, and somewhat shining granular mass, and they were usually beset with processes which anastomosed with the surrounding reticulum. In the periphery of these bodies, which, up till now, he says, have been considered to be protoplasmic in their nature, lay the so-called nuclei. Their number varied as well as their position, and many giant cells held more than a hundred. He believes that these so-called nuclei were, in reality, cells, as they undoubtedly possessed a nucleus as well as nucleolus. In the larger nodules caseation had generally taken place, and here the parts became somewhat indistinct. The elements in the giant cells were not well marked, the processes were shrivelled, they assumed a yellow aspect, and, if the giant cells had fallen out, a small cavity was produced. In the lung, liver, kidney, spleen, and various other organs he finds exactly the same structure, and, in conclusion, he says (p. 103) that the structures he found in the serous membranes were identical with those described by Wagner and Schüppel. In the parenchymatous organs the structure was identical, but was liable to be altered from caseous degeneration which very rapidly supervenes on its complete development. In the case of the pia mater he found, in one case, only a proliferation of epithelium, the lesion being of a superficial nature, but he concludes that this is merely an early stage

in the formation of true reticulated tubercle. The reticulated tubercle is situated in organs altered by inflammatory action, more especially where there is a local caseous source of infection (p. 108).

Buhl, again (*loc. cit.*), gives the following description of tubercle of the lung (p. 97). Tubercle of the lung is a new formation of microscopic size, without vessels, but surrounded by a vascular network. Fresh young tubercle shows circularly arranged fibres at the periphery, in which are small round shining nuclei, vessels here and there, and, more internally, a proper reticulum which, on account of the want of nuclei, he is inclined not to take for fibrous tissue, but for a homogeneous connective substance. This reticulum is non-vascular. The younger the tubercle, the less is this reticulum developed. He distinguishes in the tubercular nodule three kinds of cells (p. 33); (*a*) cytogenous cells; (*b*) giant-cells from which come off numerous processes, and which contain several nuclei; (*c*) cells of an epithelial type whose protoplasm is shining, finely granular, and surrounded by a membrane. The last possess large or small nuclei, which may resemble cytogenous cells, or the nuclei of giant cells. He concludes that acute miliary tuberculosis is clinically essentially a local disease, a desquamative pneumonia, which can be distinguished from the pure genuine form only by the presence of giant cells among the proliferated epithelium.

These statements have been in great part verified by Klein's observations on miliary tubercle in man ('Anatomy of the Lymphatic System').

It is apparent, therefore, that there is a certain definite structure which is, one may say, invariably met with in true tubercle, not consisting in the presence of one particular cell, but in a definite arrangement of the various elements into what may be called a *giant-cell system*, and that one of these systems does not alone always constitute a naked-eye tubercle, but that several may be, and generally are, placed in close contiguity, forming a macroscopic, grey, gelatinous deposit. In all cases there is a tendency to the formation of a concentric band of fibrous tissue at the periphery of each system, and hence the nodule, usually, has a rounded form. Such a nodule is visible to the naked eye, but there are numbers of tubercular nodules of the most typical construction, which are totally overlooked if this method of examination alone be employed; and it is evident that we can never say with certainty whether a nodule is tubercular, or whether an organ contains tubercle, until we have made a systematic, minute examination. Tubercle is a *microscopic*, not a *macroscopic* object.

All the observers quoted agree that a giant cell or several giant cells always form an integral part, and, in all probability, a most important part in the structure of a tubercle; and it will be remarked that these giant cells are not exactly identical with the

myeloid cells found in certain tumours and in bone medulla, but that they are surrounded by processes which run outwards, to divide and form a branching reticulum, or at least join with it. Recent observations go to show that the processes of the giant cells *construct* this reticulum, and that the latter subsequently joins with the concentric band of fibrous tissue at the periphery. Certain it is, at any rate, that the giant cells have enormous numbers of processes which are always to be seen where the tubercle has not undergone degeneration. Granted that the formation of a giant cell is the starting-point in the production of a tubercular nodule, it comes to be an exceedingly interesting matter to determine how it is that these giant cells originate. They were described by Virchow ('Virchow's Archiv,' xiv, 49, 1858) as occurring in tubercle of the peritoneum, but their significance was not recognised by him at this time. He also describes them in his cellular pathology (3 Aufl., p. 441), and in his work on tumours reference is also made to them. He supposed that they were formed from the connective-tissue corpuscles. Rokitsky also referred to them as mother cells in tubercle of a peritoneal pseudo membrane ('Hand-book of Pathological Anatomy'). Busch found "myeloplaxen" in tubercle of the choroid ('Archiv f. Patholog. Anat.,' xxxvi, 449, 1866).

Langhams ('Virchow's Archiv,' xlii, 382, 1868), from the examination of a large number of cases, came to the conclusion that they were a constant constituent of tubercle. He says that they very often have a cellular covering or mantle, and he supposes that they may be produced in one of two ways—either by the division of a single cell, or by the running together of several cells. These giant cells, according to Langhams, lie in the part of the tubercle where cellular structures are most abundant. Their nuclei are extremely clear and vesicular.

Klebs ('Virchow's Archiv,' xliv, 286) and Köster ('Virchow's Archiv,' xlviii, 95) represent the giant cells found in tubercle as transverse sections of capillary lymphatic vessels, and their views have been adopted by Hering (loc. cit.). Schüppel believed that they were formed within blood-vessels (loc. cit., and 'Archiv der Heilkunde,' 1868, Heft 6) by the running together of several colourless blood-corpuscles. So soon as the cells became nucleated, *i. e.* as soon as true giant cells are produced, the wall of the blood-vessel disappears. Thaon (loc. cit.) also refers to their origin within blood-vessels, and a similar mode of origin is assigned to them by Wegner and Rustitsky ('Virchow's Archiv,' lvi, 531 and 532; and lix, 217 and 224). Brodowski (loc. cit.), from extensive observations on this subject, concludes that the giant cells are in close relationship with the changes which go on in the blood-vessels of the part. These changes, however, according to him, differ

from those described by Schüppel and Wegner chiefly in this: that they take their origin not from the walls of blood-vessels whose structure is complete, but from the germs of newly developing vessels. There is no doubt that they are in connection with capillary blood-vessels, but that they are simple outgrowths from the wall of the same, as Wegner supposes, he cannot agree to. In the neighbourhood of the giant cells, structures of a similar nature are found, but smaller in size. Their shape is round, pyriform, or forked, and they are called small giant cells (*sic*) by some authors. They are all more or less filled with oval or elongated nuclei, which are placed, as in the larger giant cells, at the periphery. The latter are also smoother than the large giant cells, and they sometimes have processes running off from them. They are placed in spaces which frequently surround blood-vessels (perivascular spaces of His), and in this respect also resemble true giant cells.

Their reaction with staining reagents is also similar to the latter, and a gradual transformation from one kind of cell to the other is frequently met with. He found this last condition best marked in so-called tubercle of the spleen, in the medulla of bone, and in phthisical lungs. He says that one can hardly doubt the formation of large giant cells out of these smaller and evidently younger cells. What is chiefly remarkable, however, in Brodowski's paper on the subject is the connection which he traces between these smaller bodies and newly formed blood-vessels. They are connected to the blood-vessels by a long protoplasmic process similar to that described by Arnold ('Virchow's Archiv,' liii, 70; liv, 616), Leuschin ('Melanges Biologiques de l'Academie de St. Petersburg,' vol. viii, 307 and 316), and Rouget ('Archives de Physiologie normale et pathologique,' 1873, 60), as characteristic of the mode of formation of young blood-vessels. Leuschin has been able to inject such processes. They communicated with a capillary on the one hand and a myeloplax on the other. Brodowski believes that the giant cells result from an abnormal functional capability of the vascular walls, manifested in a superabundant proliferation of their nuclei. His observations led him to the conclusion that the reticulum found round the giant cell in tubercle is protoplasmic in character, and identical in its nature with the processes of the giant cells themselves. It is nucleated in many places, and, in others, contains vacuoles. He believes that many of these processes are hollow, and that they communicate with the vacuoles which are so frequently found in the giant cells. He will, however, not say that the formation of giant cells always takes place from blood-vessels. It is quite possible, for instance, that they may be formed in connection with lymphatics, although such a connection, in the manner above indicated, has not been traced.

A somewhat similar relation between giant cells and newly formed

blood-vessels seems, to a certain extent, corroborated by Ziegler's experiments, consisting in the introduction of two contiguous glass plates under the skin of animals (*loc. cit.*). He finds that the giant cells are not merely connected with blood-vessels, but that they form the constructive material not only for blood-vessels but also for connective tissue in granulating wounds.

In all granulating wounds giant cells are abundant, according to Buhl; but, more especially, in those of a scrofulous nature ("Das Granulations-gewebe und seine Bedeutung für die Scrofulose," 'Wiener Med. Jahrb.,' 1876, S. 157); and, according to him, they subsequently form lymphoid corpuscles or epithelial cells. Similarly, Jacobson has found giant cells abundant in healthy granulating wounds ('Virchow's Archiv,' lxxv, 120). They do not lie in the pyogenic, but in the protoplasmic area; and Billroth described bodies of the same kind in fungating ulcers ('Untersuchungen über die Entwicklung der Blutgefäße,' Berlin, 1856, S. 32).

Klebs has also found them in Elephantiasis Arabum, Brodowski in two cases of lupus, and Friedländer also describes them in lupus ('Virchow's Archiv,' lx, h. 1); Köster likewise enumerates many other situations in which they occur ('Centralblatt,' 1873, No. 58).

In no case, however, does one find a reticulum with connective fibres at the periphery unless in true tubercle; and we are warranted in concluding, therefore, that the structure of a true tubercle does not merely consist in the presence of one or more giant cells, but in their peculiar connection with a circumscribed reticulum containing lymphoid and epithelioid cells in its meshes. Such a structure we believe to have been seen in all tubercular formations in man; so much so that where it is not met with one should be extremely sceptical as to the new formation being *true tubercle*. The nodules may be so altered by various degenerations that the typical arrangement of parts may be lost; but, if careful search be made, we are convinced that such a structure will always be found typically developed in certain situations.

Such being then the admitted structure of a true tubercle, what relation does it bear to special organs, and how is its development brought about in them? There is no situation in which tubercle occurs with such great frequency, and in which the structure is so complex, as in the lung. It is here that from the earliest times the true nature of tubercle has been sought for, and there is no organ in which lesions widely different have been so much confounded.

It is to the labours of Niemeyer that we are mainly indebted for the great differentiation between diseases due to a mere chronic catarrhal affection and those dependent on a truly tubercular new formation. The pathology of ordinary phthisis of the lung has, since his views were published, become intelligible, and is found

practically to coincide with the usual clinical history. It is not so much with this catarrhal condition that we have to do at present as with what is known as "miliary tubercle of the lung." Up till very lately the majority of physicians and pathologists have looked upon the terms "miliary tubercle of the lung" and "interstitial tubercle of the lung" as synonymous; and many, even yet, we believe, entertain the same views. In other words, those grey gelatinous deposits which occur in the lungs of children in general tuberculosis have somewhat rashly been placed in the category of interstitial tubercle. This, no doubt, is due to these nodules occurring as part of a general disease, where the deposits in other organs were supposed to be of the interstitial variety. Before going any further, we would ask what is meant by the term "interstitial?" It no doubt arose from the doctrines of connective-tissue proliferation formerly taught by Virchow, and referred to a new formation, or a change of a proliferative nature which took place in the connective tissue of the part; but, in later times, the term *interstitial* has been employed in a very loose manner, without any particular reference to its original signification. If, for instance, a tubercular tumour can be proved to arise from the endothelium of a blood-vessel or lymphatic, are we to call this an interstitial tubercle? If so, why not then call a catarrhal pneumonia, taking its origin in the alveolar epithelium, also an interstitial change? Neither arise in the truly interstitial tissue, and, in all probability, only affect the interstitial tissue secondarily; and why they should be called *interstitial* seems to us totally inexplicable. The whole tendency of modern pathological research is to show that, in all probability, true tubercle *never* arises in the interstitial connective tissue; but, like cancer, taking its origin from an *epithelial* surface, is constantly developed from an *endothelial* surface. If this be true, can any form of tubercle be properly called interstitial?

In regard to true miliary tubercle of the lung, such as is found in general tuberculosis of children, all modern researches go to show that it springs in the first instance from the epithelium lining the alveolar cavities. This layer of epithelial cells is single, flat, and corresponds to an endothelium in regard to its general appearance and connections with underlying parts. In fact, had we the alveolar walls spread out flatly and continuously, the tissue consisting of the alveolar wall proper, covered with its epithelium and underlying plasma spaces, would almost exactly resemble one of the serous membranes. Buhl (*loc. cit.*) looks upon "acute miliary tuberculosis" as a desquamative pneumonia (lobular catarrhal pneumonia), which only differs from the ordinary form in that within the alveoli, in addition to the ordinary epithelial elements, giant cells are formed. *The tubercle is primarily situated within the alveolar cavity.* Hering (*loc. cit.*) makes a similar

assertion, and declares it as his belief that those cases generally described as acute miliary tuberculosis, and distinguished by their rapid course, are caused by disseminated acute catarrhal pneumonia. Klein ('Serous Membranes,' p. 71) states that all such cases in man have, at first, the character of desquamative pneumonia. Buhl's assertion that the essential difference between this and genuine desquamative pneumonia is the presence of giant cells among the germinating epithelium, only corresponds to a certain stage of development of the tubercles. He has found, in two out of seven cases, the air cells distended with a fibrinous material containing granules and small cells (p. 72). He grants, however, that though giant cells are not present at first, they are always present in tubercles of older standing. That this material spoken about is fibrinous in its nature we are inclined to doubt. It certainly, in the drawings given of it, has no resemblance to fibrin, but is much more like a form of colloid, which is seen with great frequency in tubercular lungs of man, and which has been more especially figured and described by Thaon (*loc. cit.*). The nodules, however, are essentially made up of distended groups of alveoli, round which is a variable amount of cellular infiltration. Then the interalveolar tissue afterwards becomes thickened and infiltrated with lymphoid cells, and the capillary vessels are not entirely permeable. In some nodules only the central alveoli are distended with this so-called fibrin. Those at the periphery are occupied in one or other of the following ways: (1) by spherical or irregularly shaped nucleated elements, many of which can be recognised as epithelial in their nature by their size, granulation, and nucleus; (2) giant cells generally imbedded in or connected with retiform interalveolar tissue. Thus the appearance of a reticular giant-cell tubercle is produced. Those appearances included under the second head he considers to be a further advanced stage of those described under the first, the fibrinous exudation gradually disappearing and becoming replaced by groups of cells (most of them the offspring of the alveolar epithelium), or by one multinuclear mass or giant cell (p. 73). He looks upon the alveolar epithelium as the source of the giant cells (p. 76).

It is, therefore, clear that miliary tubercle of the lung is certainly not an interstitial form, but traces its origin to a germination of the alveolar epithelium. It also, throughout its course, is mainly placed within the alveolar cavities, and only involves the alveolar wall secondarily. What, then, is the difference between true miliary tubercle and mere catarrhal pneumonia? Is there any difference, and, if so, in what does it consist? We are convinced that there certainly is the most marked difference between the two. Both arise in the same way and at one period, an extremely early one, they do not much differ in structure. In both the alveolar cavities are filled

with the cells resulting from epithelial germination. Soon, however, the differentiation shows itself. The catarrhal pneumonia tends to degenerate and become caseous, the alveolar walls break down, and a small phthisical cavity results. In the case of the miliary tubercle, however, there is no tendency at this period of the disease to degeneration; but, on the contrary, a *formative process* ensues, in which we have very soon one or more of the proliferated epithelial cells enlarging, becoming granular, and producing a giant cell. From this, giant cell processes are rapidly thrown out, forming a reticulum which is bounded by the alveolar wall. The whole alveolar wall then becomes incorporated with the growth, and aids in producing a reticulated tubercle.

Of course, no one at the present day believes that the yellow caseous masses placed in the line of a bronchus, and occurring in phthisis of the lung, are true tubercle, or are tubercular at all in their origin; but there is the possibility that in such lungs veritable reticulated tubercle may be secondarily developed from the caseous infecting deposit in the neighbourhood. Niemeyer ('Clinical Lectures on Pulmonary Consumption,' 1870) has exactly described, in his masterly work, the condition of parts in those cases. He says—

"In many cases there is not a single tubercle found in phthisical lungs, and the consolidation and destruction of the lung are caused alone by inflammation leading to induration and subsequent softening. Virchow, however, goes too far when he says that almost all the alleged miliary tubercles of the lungs are in reality bronchitic, peribronchitic, and pneumonic deposits. The grey nodules occurring in miliary tuberculosis, and of whose tubercular nature there can be no doubt, are frequently met with in *phthisical* lungs. In pulmonary phthisis the tubercles, in the majority of cases, are clearly of recent origin, and have complicated the pulmonary phthisis when it was already in an advanced stage" (p. 3).

He adds (p. 11) that the greatest danger to most phthisical patients is the development of tubercles. If tubercles are formed in lungs the subject of phthisis, they are, almost invariably, seen in the neighbourhood of cavities, and they stretch out from this. Tuberculosis is in most cases a secondary disease, originating in some way unknown to us in the action of cheesy morbid products on the organism. He believes much more in a local process, in which the lymphatics probably take great part, than in a general poison infecting the blood, as has been held by Buhl.

We are convinced that this description by Niemeyer is strictly correct. *Tubercle is frequently produced in phthisical lungs*, and we also believe that it is in the neighbourhood of cavities that structures of this kind will be seen. Its primary seat of development is, however, different from that of the miliary tubercle

of children. In the latter case it was seen to be an intra-alveolar structure; but here, in the ordinary phthisical lung, we believe that no such mode of origin is to be seen, but that, in all probability, it is within the perivascular and peribronchial lymphatics that it originates. It may, we consider, be accepted as generally true, that if tubercle develops secondarily in an organ, in the neighbourhood of a cheesy deposit, it is within the lymphatic vessels of the part, and specially to their endothelium, that we have to look for its source. In general tuberculosis of children the affection of the lung is probably, in the great majority of cases, the primary one; the other organs, such as the spleen, kidney, and liver, being secondarily implicated, and, most evidently so, by means of the blood-vessels.

In ordinary phthisis of the lung, however, arising from a chronic catarrhal pneumonia, the tubercle in the lung is locally produced, and is brought about from the absorption of the caseous débris by the neighbouring lymphatics. As Niemeyer has also most truly remarked, in those cases of chronic phthisis, one of the commonest seats of secondary tubercle is in the pleura, and from what we know of the distribution of the perivascular lymphatics this is the situation in which we would *à priori* have expected to find it.

We are inclined to believe that tubercle of the kidney is invariably secondary to a tubercular or cheesy deposit elsewhere, and that the condition known as the "phthisical kidney," found in genito-urinary phthisis, is not in reality tubercular, but merely corresponds to catarrhal pneumonia in its mode of origin, course, and termination in the formation of phthisical cavities. It is essentially a chronic renal catarrh, in which the pressure of accumulated material is so great that the nourishment of the part becomes impaired, and it falls into a state of caseous decay. It cannot, however, be denied that this lesion, as in the chronic caseous catarrh of the lung, may give rise to true tubercle in its neighbourhood.

This, however, is exceptional; the course of the disease is usually uncomplicated with a truly tubercular formation. Miliary tubercle of the kidney, occurring in children as part of a general affection, is, however, tubercular from the first, and, without doubt, as Schüppel (*loc. cit.*) has truly remarked, takes its origin from the blood-vessels. It is brought to the kidney in the shape of caseous material, or cells having a peculiar activity of growth, and produces in the endothelium of the smaller renal arteries a local proliferation. The organs in which secondary tubercle is deposited, presuming that the lung is the primary source, are those in which we find emboli most commonly lodged, namely, the spleen and kidney. This fact, therefore, gives support to the arterial origin of tubercle in these organs, and a minuter examination verifies the assertion of what, at first sight, seems an extremely probable channel of infection.

Germination having arisen within a small branch of one of the

main stems of the renal artery, the vessel rapidly becomes occluded, and its wall obliterated. The same structure is found here as in other organs, consisting of a true reticulated tubercle; but a source of fallacy that one must be guarded against is that the typical structure of the giant cell and reticulum rapidly becomes obliterated from the caseous transformation which sets in at a very early period in the development of the nodule.

We cannot here, at any length, enter into the consideration of tubercle as it occurs in the spleen and liver. Suffice it to say, however, that the latest and most accurate researches go to show that it has a similar origin from the endothelium of the arterial walls, or from that of a lymphatic vessel, never, so far as we know, primarily from the interstitial stroma. So that the name interstitial tubercle can be used only in a very restricted sense, and only as a significant term to indicate what we know as true tubercle. Another fact ought always to be borne in mind—that in any organ where we have a series of ducts entering into its structure there may arise a catarrhal affection which might, by becoming caseous, closely resemble a tubercular new formation. Phthisical cavities are produced in such organs from molecular disintegration, but that these affections are tubercular there is not the slightest evidence to show. It is their resemblance to ordinary catarrhal phthisis in the lung that has led to their being mistaken for tubercle, but now that we know the true nature of the latter affection such a mistake ought not to be permitted in our pathological nomenclature.

The degenerations to which tubercle is liable have been more carefully studied lately than they were before. By far the commonest is the *caseous metamorphosis*, depending, in all probability, on an early implication of the blood-vessels, and diminished blood-vascular supply. We believe it is a fact, borne out by the experience of all pathologists, that in general tuberculosis of children there is less tendency for miliary tubercle of the lung to become caseous than that placed in other organs, such as the kidney, liver, and spleen. We refer of course to a truly miliary deposit scattered diffusely throughout the lung tissue. And, granting that in this organ the nodules take their origin from the alveolar epithelium, it is some time before the vessels supplying such tubercles become implicated in the new growth, and their lumina occluded. Supposing, however, that the new formations in other organs arise in the blood-vessels, from their endothelium, and that their lumina rapidly become filled up with proliferated cells, one can easily see how the tubercle should become caseous at an early stage of its growth. One of the essentials in the formation of the tumour is a filling-up of the vascular lumen followed by a transformation of the arterial wall. Hence the nourishment of the part is impaired, and it rapidly falls into a state of caseous decay. We believe that the *fibrous trans-*

formation spoken of by Virchow is more commonly associated with caseous catarrhal affections than with true tubercle, and, where it does occur as a complication of a truly tubercular growth, we are inclined to believe that the fibrous formation is primary and the tubercular secondary.

There is another degenerative product which we believe was originally pointed out by Laennec, but which has been more especially investigated by Thaon (loc. cit.). Laennec described it as a jelly-like tuberculous matter sometimes found in the neighbourhood of miliary granulations. It might be mistaken for œdema, but the alveolar structure is indistinctly visible in it. The true nature of this deposit, however, has been described by Thaon as a "caseous colloid pneumonia." The lung, in those cases, is bound to the side, infiltrated with a trembling material, which soon becomes firmer in consistence, grey, and at last yellowish-white in colour. It may exist as a degeneration in various affections of the lung, more especially certain forms of catarrhal pneumonia where it is associated with a truly tubercular deposit. The tubercular nodules themselves also participate in the degeneration. The material which is formed is exactly similar to the colloid found in the thyroid gland and elsewhere. We believe that what Klein (loc. cit.) has described as a fibrinous deposit in certain forms of tubercle of the lung is in reality this substance. It is produced by a true colloid degeneration of cells, and we also feel persuaded that what are figured by him as enormous giant cells, occupying the entire alveolus, and without any distinct processes or reticulum, are, in reality, masses of this substance with the contained cells undergoing the colloid transformation. True giant cells never have this appearance; they are always much more granular, and do not form an accurate cast of the alveolar cavities, as this substance does.

In conclusion, we would merely call attention to the absurdity and incongruity which exist in the nomenclature of tubercle. Even in our latest English text-book of pathological anatomy (Jones and Sieveking's 'Pathological Anatomy,' Payne, 1875), we find that tubercle is spoken of as being of two forms, grey and yellow. And, no doubt, this, even at the present day, must be so far accepted as a classification which answers general purposes; and, provided that we understand the meaning and significance of the two terms, little harm may come of it. What, however, can be more misleading to the student of pathology than a classification of this kind, where he is led to suppose, from the prominence given to the name *yellow tubercle*, that it is a special form instead of a mere degeneration of the grey variety; or, what is much more common, not a tubercle whatever. We might as well speak of a grey or yellow cancer; nay, we could even speak of this in a more accurate sense than we do of the other new formation, seeing that a cancerous new formation is not so

liable to be mistaken for a structure resembling it, as is the case with tubercle. If we are to have anything like a scientific classification of morbid lesions, and if pathology is to become an accurate science, we must go back to first principles and build on a basis not made up of old exploded ideas. Our recent methods of investigation enable us to go thus far. We can trace morbid lesions to their structural origin. It is not in this that the future field for speculation lies, but more in what might be called the physiology of pathological anatomy. The sooner, therefore, we give up a nomenclature founded, in great part, upon mere theory the better. We are quite in a position to do so, and why we should still hold by terms which merely serve to create confusion, where all should be clear and accurate, seems somewhat inexplicable.

VII.—The Morbid Anatomy of the Insane Brain.¹

NEARLY forty years ago the writer of an article in this Review,² commenting upon some lectures on insanity published by Mr. W. A. F. Browne, and irritated by the evident unwillingness of the author to admit of any form of disease without an anatomical basis, exclaims, "When it is proved that the nervous system has no actions which the eye cannot follow, no conditions which are not appreciable by sight or touch, we may expect to find every symptom explained by hardness or softness, by redness or paleness, by hypertrophy or wasting, by extravasation or ulceration, but not before. The assertion of invariable palpable lesion of structure, whenever there is lesion of function, is no more, at present, than the jargon of a school which dreads the admission of anything that is not physical, and, on the ground of this miserable and narrow view,

¹ 1. *Archives de Physiologie*, 1873.

2. *Ibid.*, 1875. *Études sur les lésions cérébrale dans la paralysie générale.* Par M. MIERZEJEWSKI.

3. *West Riding Asylum Reports*, 1874. *Histology of the Morbid Brain.* By HERBERT C. MAJOR.

4. *Quarterly Journal of Mental Science*, 1874. *Notes on a case of Senile Dementia.* By JAMES HOWDEN, M.D.

5. *Ibid.* *Remarks on a Case of Syphilitic Insanity.* By J. BATTY TUXE, M.D.

6. *Ibid.*, 1875. *Two Cases of Lesion of the Pons Varolii.* By J. J. BROWN, M.B.

7. *The Morisonian Lectures for 1874.*

8. *American Journal of Insanity*, 1874. *Morbid appearances in Insane Brains.* By Dr. J. P. GRAY.

9. *Allgemeine Zeitschrift für Psychiatrie*, 1875.

10. *Archiv für Psychiatrie*, 1875.

² I, 1838.

aspires to the dignity of a superior philosophy." We can hardly estimate how much faith was required at that time to help pathologists to hold fast the principle "*physica physiciis demonstranda*," especially in this problem of insanity. The microscope was then little understood or applied to morbid anatomy, and the delicate physical experiments of modern physiologists were not even dreamt of. "They are ill discoverers that think there is no land when they see nothing but sea," says Bacon,¹ yet it was long before much was added to the "hardness or softness, hypertrophy or wasting," of the older pathologists; even twenty years later, Drs. Bucknill and Tuke,² in the first edition of their manual, confessed that the microscope afforded them "no distinction between the sane and the insane brain." Neuro-pathology, generally, was much in the same condition, for though additions had been made to the scanty nosology of Cullen, the first half of the present century passed without much having been done either in clinical or pathological work; it was, indeed, just about the time of the publication of Bucknill and Tuke's manual that a sort of revival commenced, ushered in by the labours of Duchenne and others. Locomotor ataxy, though previously described by Romberg in 1848-51, was re-described and rendered generally known by Duchenne in 1858; progressive muscular atrophy was defined by Cruveilhier in 1858, and labio-glosso-laryngeal paralysis was added by Duchenne in 1860. The microscope, now perfected, in the hands of skilled observers like Vulpian, Luys, and Charcot, has since given to each of those diseases a local morbid anatomy. The problems of insanity remained without further elucidation for a while. The anatomy and physiology of the spinal cord, medulla oblongata, and great cerebral ganglia, yielded themselves more easily to the investigations of anatomists and physiologists, and a mass of knowledge had been accumulated which bore so directly upon clinical and pathological phenomena, that great strides became possible in the pathology of these organs. The microscopic anatomy of the cortex soon became known, but all attempts to investigate the functions of the grey matter seemed hopeless; it was thought to be destitute of sensation, and not capable of even electrical stimulation. Gall and Spurzheim's ideas had fallen into general discredit, and the only piece of physiology we possessed depended upon the disputed views of Broca as to the function of the third left cerebral convolution, and this rested on purely pathological grounds. In course of time Lockhart Clarke, Meschede, Poincaré, and Bonnet, all described granular degeneration of the cells of the cortex in the brains of the insane; and in 1868 Drs. Batty Tuke and Rutherford published an

¹ 'Advancement of Learning,' Book II, vii, 5.

² 'Manual of Psychological Medicine.' London, 1858.

account of their examination of the brains in thirty cases of insanity, the result of which was to show that changes take place in the blood-vessels, nerve-cells, fibres, and neuroglia in a large number of cases, perhaps in all chronic cases. In 1870, Hitzig¹ published his first paper on the effects of the electrical stimulation of the brain cortex; his experiments are now so well known that we need only say that he succeeded in localising to some extent muscular movements in definite areas of the surface of brain. In the last seven years every department of our knowledge of the brain has been largely increased; the dissections of Meynert,² Charcot,³ and Broadbent⁴ have traced the arrangement of the nerve-fibres as they pass from the peripheral nerves to centres in the cerebral ganglia and hemispherical grey matter, the connections of the centres with each other, and the course of the outgoing fibres to the nerves again. Duret⁵ and Heubner⁶ have elaborated the meagre description of Willis, and have completed our knowledge of the general arrangement of the cerebral vessels; Key and Retzius⁷ have done as much or more to enlarge our knowledge of the lymphatic system; Ranvier,⁸ Boll,⁹ and Mierzejewski,¹⁰ have elucidated the structure of the neuroglia and the formation of the medullary sheaths; Turner,¹¹ and especially Ecker¹² in his monograph, have familiarised us with the previously obscure arrangement of the cerebral convolutions, while Féré¹³ has drawn attention to the relations of cerebral topography to the external configuration of the cranium. In physiology, Hitzig¹⁴ has followed up his previous observations; Ferrier¹⁵ has successfully originated departures from Hitzig's method, and has succeeded in localising centres for various groups of muscles, for sight, hearing, smell, taste, and for common and organic sensations which have been in the main confirmed by Carville, Duret and others; Nothnagel,¹⁶ by his method of destroying limited parts of the brain,

¹ Du Bois-Reymond's 'Archiv,' 1870.

² Stricker's 'Handbook of Histology.'

³ 'Progrès Médical,' 1874-5-6.

⁴ 'Journal of Mental Science,' 1870.

⁵ 'Archives de Physiologie,' 1874.

⁶ 'Die Luetische Erkrankung der Hirnarterien.' Leipzig, 1874.

⁷ 'Study in the Anatomy of the Nervous System, and of the Connective Tissue.' Stockholm, 1876.

⁸ 'Archives de Physiologie,' 1873.

⁹ 'Archiv für Psychiatrie,' 1873.

¹⁰ 'Archives de Physiologie,' 1875.

¹¹ 'West Riding Asylum Reports,' 1873, and 'Journal of Anatomy and Physiology,' 1870.

¹² 'The Convolutions of the Human Brain.' London, 1873.

¹³ 'Archives de Physiologie,' 1876.

¹⁴ 'Untersuchungen über das Gehirn Abhandlungen Physiologischen Inhalts.' Berlin, 1874.

¹⁵ 'West Riding Reports;' 'Trans. of Royal Society,' and 'The Functions of the Brain,' London, 1876.

¹⁶ 'Virchow's Archiv,' 1874-75-76.

has added weighty evidence to the general theory of localisation; Gudden,¹ by operating on the peripheral termination of nerves in newly born animals, has found resulting atrophies in limited parts of the brain, and by his experiments supports the views of Hitzig and Ferrier.² Dr. Otto Soltmann³ adds that the brains of newborn animals do not react to the electrical stimulus, but that the experiment depends upon the proper development of the cells of the cortex.

Schiff⁴ has shown that heat is given out in the brain on its reception of a peripheral stimulus through the nerves; Rochefontaine⁵ and Brown-Séquard⁶ have demonstrated the influence of the brain cortex on the vaso-motor system; Lépine⁷ has described its influence on the heart-beats; lastly, Eulenburg and Landois⁸ have localised thermal centres in the anterior lobes. The grey matter of the hemispheres is no longer beyond the reach of physical investigation.

In the meantime, to return to insanity, we find that after 1870 general observations upon the microscopical condition of the brains of the insane accumulated so fast that, in 1873, Dr. Batty Tuke presented a summary of the results then attained in a series of papers published in vols. lxi and lxii of this Review, and in the new edition of Bucknill and Tuke's 'Manual' a section devoted to morbid histology was contributed by the same careful observer. These observations only extended to chronic cases; no attempt was made to estimate their value, and but little had been done towards establishing distinct pathological entities. We propose to consider the contributions which have been made, since Dr. Tuke wrote his articles, towards defining such entities, and we have to regret that more has not been done in this direction; that more special classes of cases have not been worked at. We hope to see all our great English asylums contributing towards this important work; much labour is required; a careful microscopical examination of even one case involves many weeks' work if it is done well, and the preliminary training before sufficient skill is acquired is slow and tedious, but the results are encouraging enough to make it worth while: our asylum physicians are not usually over-worked members of our profession; the younger ones especially have the time and ability to add largely to our knowledge. Of the

¹ 'Archiv für Psychiatrie,' Bd. ii.

² 'Centralblatt,' Nos. 8 and 9, 1875.

³ 'Jahrbuch der Kinderheilkunde,' Band ix.

⁴ 'Archives de Physiologie,' ii and iii.

⁵ 'Gaz. des Hôpitaux.' 1875.

⁶ 'Archives de Physiologie.' 1875.

⁷ 'Gaz. de Hôpitaux.' 1875.

⁸ 'Virchow's Archiv.' 1876.

researches made to establish definite pathological entities, the most successful, without doubt, have been the contributions to the morbid anatomy of general paralysis; this is not to be wondered at, for it has been since the time of Bayle, the type of insanity to which the physical school has always referred in support of their doctrine; still there have been doubts as to the identity of the morbid process, even Bucknill and Tuke in their last edition treat it as a complication, giving it no distinct place in their classification, and they say "no change in the brain or its membranes has yet been demonstrated which is special to general paralysis."¹ Dr. Batty Tuke² considers it entitled to the position of a pathological entity; and Dr. Clouston in 'Skae's Morisonian Lectures' for 1873³ calls it "a true pathological disease of the nervous system as distinct from all other varieties of insanity and all other nervous diseases as smallpox is from scarlatina, or consumption is from rheumatism." The principal papers on this subject are those of Magnan and Mierzejewski,⁴ Lubimoff,⁵ Mierzejewski,⁶ and Ludwig Meyer.⁷ Mierzejewski examined eighteen cases of this disease. Meyer gives the results of thirty cases. The following is a digest taken principally from M. Mierzejewski's articles. He divides his observations as follows:

- I. The lesions of the vessels.
- II. Those of the interstitial substance or neuroglia.
- III. Those of the nervous elements.

1. *Vessels*.—These lesions are the most constant and precede the more profound affections of the nervous tissues. The vessels are not everywhere equally affected, but the disease is general throughout the brain. In the earliest stages there is an increase of the nuclei in the walls of the capillaries, and clustering of nuclei under the adventitia of the small vessels; Dr. Meyer believes that these are actually cells, not nuclei, in the walls of the vessels; later, miliary aneurisms, extravasations of blood, and rupture of the vascular walls occur frequently; the capillaries become thickened and their walls assume a homogeneous glass-like aspect; at last the vessels undergo general fatty degeneration. New vessels are formed, he believes, as the capillaries present appendages like those described in the growing tail of the tadpole, and he thinks the branching cells present in this disease, take part in their formation. The nuclei

¹ 'Manual of Psychological Medicine.' London, 1874, p. 610.

² 'The Morisonian Lectures.' Edin., 1874.

³ 'Journal of Mental Science,' 1875.

⁴ 'Archives de Physiologie,' 1873.

⁵ 'Virchow's Archiv,' Bd. 4.

⁶ 'Archives de Physiologie,' 1875.

⁷ 'Zeitschrift für Psychiatrie,' 1875.

are chiefly augmented in the adventitia, but the transverse and longitudinal nuclei of the middle and inner coats also participate in the increase; he believes this to be due to the wandering of leucocytes into the coats of the vessel; sometimes red corpuscles accompany them, and there are always present chronic inflammatory processes and deposits of pigment granules in the adventitia. The vessels become twisted, and serpentine within their adventitious coats. These chronic inflammatory changes lead to the formation of miliary and dissecting aneurisms; the miliary aneurisms are such as are described by Bouchard and Charcot;¹ those he met with were usually fusiform. Extra-parietal hæmorrhages he found only in one case, but in that they were numerous and presented all stages of organisation, and the vessels were in a more or less advanced condition of fatty degeneration. He describes two kinds of fatty degeneration, one acute and primary, occurring *pari passu* with an active inflammatory condition of the walls; the other secondary, following upon a chronic inflammatory process. A spiny or furred condition of the capillaries has been often described; it is produced in some cases by their having numerous filiform appendages, in others from their possessing bundles of fine fibres united to them by a granular amorphous material, or by the prolongations of the cells of the interstitial tissue touching their walls and so giving rise to this appearance.

2. *Interstitial tissue*.—These changes he divides into three periods corresponding to the three developmental phases of the process, all of which may be seen at one time in the same brain.

In the first stage there is a considerable augmentation of the nuclei of the interstitial tissue, sometimes as much as double the usual number, often occurring in foci; some of the nuclei are oblong with median fissures, some are laterally constricted and some are arranged in rows. He has observed them to present three forms; the first is round, readily coloured by carmine, with a double contour; the second is oval, coloured feebly by carmine, showing a single contour and finely granular; the third is oblong or rod-shaped with lateral constrictions in some cases, in others having little projecting spines, in others again bent like a horseshoe: these last being observed very often near the blood-vessels.

In the second stage, the white matter of the hemispheres is invaded by large islands of amorphous, homogeneous, opaque matter, of irregular firm, unequal surface, presenting many vacuolations. The islands are badly defined at their margins, and take up carmine readily. Many nuclei seem to be imprisoned in their interior, but some parts are without nuclei; filaments from these masses radiate or form a free network in the interstitial tissue. Sometimes the

¹ 'Archives de Physiologie,' 1868.

seat of these masses corresponds to the course of a blood-vessel. In more advanced cases they may be observed to be undergoing contraction; their contours are well-defined; the radiating filaments are changed into uniform prolongations; some imprisoned nuclei still remain; and in general these masses have the aspect of what are called stellate cells. He considers these masses are not distinct histological units, but elements composed of connective-tissue nuclei soldered together by coagulated fibrin; it is only rarely that a true stellate cell can be found. Sometimes the nerve-cells themselves are surrounded by a network of fibrin, but this is rare.

In the third stage, atrophy of the nuclei of the interstitial tissue may reach a very high degree; their outlines become angular and their size is much diminished.

3. *Nerve-cells and fibres.*—The changes in the nervous elements are not confined to any special locality, but are generally diffused throughout the convolutions of the hemispheres. One of the principal changes he observed was in the cells near to vessels which presented sub-adventitial extravasations; in these places the nuclei of the interstitial tissue were always increased in number, and surrounded the nerve-cells, sometimes touching their surfaces; the cells thus surrounded were also imprisoned in a fine band of fibrine; they were surrounded by true vacuoles, and were increased in size; at other times they were slenderer than normal; their protoplasm appeared dim, less transparent, and the whole cell coloured strongly with carmine. He did not observe division or multiplication of their nuclei. As a further stage, the cells lost their fibrillated structure, and became filled with brown or yellowish molecules; and their nuclei lost their consistence and became disintegrated; the disintegration of the nuclei commenced centrally and spread peripherally; the cells became completely filled with a yellowish-brown pigment which was insoluble in ether, and they lost by degrees their power of taking up the carmine staining.

In other cases, near vessels and in parts of the brain which bore traces of extravasations outside the walls of the vessels, he saw the nerve-cells increased in volume, and filled with pale granular matter soluble in ether. These cells, thus degenerated, did not stain with carmine. Their prolongations underwent a similar destructive metamorphosis; but the axial cylinder prolongation showed itself the most resistant. In the occipital and frontal lobes he found oval bodies without cellular structure, readily coloured by carmine and not giving the amyloid reaction. These bodies in lengthening themselves took the form of a ribbon, which presented the chemical and optical characters of the bodies themselves, and showed varicose enlargements in its course; they could be followed some distance. Sometimes the oval bodies were alone, sometimes the ribbons were without the oval bodies; he considers them hypertrophied axis-cylin-

ders and the oval bodies as broken pieces of axis-cylinders which have become varicose as described.

These descriptions warrant our assertion that the lesion in general paralysis is a chronic diffuse interstitial encephalitis, which both clinically and pathologically is allied to the sclerosis of the posterior columns of the cord productive of locomotor ataxy. The view of Poincaré and Bonnet, that its real origin is in a primary affection of the sympathetic ganglia, receives some support from the hyperæmic condition of those structures in the neck, and the granular degeneration of their nerve-cells; but on this head further observations are wanted. Now that we know that the brain cortex presides over the sympathetic system, it is quite permissible to regard these changes in the ganglia as secondary to those in the brain; certainly the symptoms do not point to any primary affections of these structures. The etiology of all forms of sclerosis is at present extremely obscure; but, as a rule, there is greater probability in favour of regarding them as due to irritation, arising locally or propagated from other parts through sensory nerves, than that they originate in the sympathetic centres. Even if we took the view that these ganglia were first diseased, we should have to inquire how that occurred; and the result would probably be that we should fall back upon some irritation acting upon them through the blood or the nerves. At present the whole question of the etiology of morbid processes is exceedingly difficult, and any suggestions made in one disease apply to all the other changes in the brain characterised by hyperæmia. Brain pathology is not in this respect behind that of other organs; probably more accurate observations on diathetic tendencies, as suggested by Professor Laycock, will, in the future, give us the key to the true explanation of the different results of apparently similar exciting causes in different individuals.

The next group of cases which has had assigned to it a definite morbid anatomy is that known as senile insanity, or senile dementia. Bucknill and Tuke¹ speak of it as a variety of dementia, differing little in its symptoms from the chronic form; they look upon it as the result of primary disease in the encephalic centres;² but Dr. Batty Tuke, in the Morisonian lectures for 1874,³ refers it to an evolutionary condition by no means limited to the nervous system; "in fact," he says, "to the morbid conditions of the blood-vessels must be ascribed the degeneration of cellular activity and of brain tissue." The most important contributions to the morbid anatomy of this disease have been a paper by Dr. Wille⁴ and

¹ 'Manual of Psychological Medicine,' 1876, p. 200.

² *Ibid.*, p. 318.

³ 'The Morisonian Lectures' for 1874, p. 45.

⁴ 'Allgemeine Zeitschrift für Psychiatric,' xxx Bd., III heft.

Dr. Herbert Major's well-known article,¹ to which may be added a case reported by Dr. Howden in the 'Journal of Mental Science' for January, 1875.

The following short account is taken mainly from Dr. Major's article, but the other papers are quoted in addition; in the main facts all three are in perfect accord.

The Vessels.—Wille says the arteries are generally atheromatous; they were in Howden's case, but Dr. Major does not refer to this. He says the arteries and capillaries were found dilated; the small arteries showed deposits of minute granules and crystals of hæmatin in their walls, but their coats were not coarse or thickened; the perivascular spaces were dilated and the surrounding brain-tissue was atrophied. Dr. Howden describes miliary aneurisms on the vessels of the pia mater in his case.

The Cells.—Changes in these can be demonstrated throughout the entire depth of the grey matter, but the process is most distinct in the large pyramidal cells lying midway in the cortical substance, whilst the round or oval cells participate least in the morbid process; the frontal and parietal regions show the conditions most distinctly, the occipital and inferior convolutions being affected to a slighter extent, more especially at the tip of the occipital lobes.

The morbid process is a granular degeneration going on to complete molecular disintegration of the nerve-cells. In the first stage, as observed in the large pyramidal cells, these bodies lose their sharply-defined outline, and become swollen; the prolongation of the cell towards the periphery remains distinct, but the others are destroyed, so as to give the cell a rounded-off appearance; the nucleus becomes swollen, and the nucleoli appear with greater distinctness; the natural pigment of cell disappears in part or completely, and both cell and nucleus cease to take up the colouring agent readily, although this property is retained by the nucleolus. In the second stage a deposit of granules appears upon or within the protoplasm of the cell, giving it a more or less opaque appearance. Sometimes the granules are collected in a heap placed excentrically, pushing the nucleus to one side, and deforming the cell contour. Lastly, the whole cell becomes completely filled with yellow granules, shrinks, and breaks down, leaving its nucleus surrounded by a heap of granules. In the third stage these granules disappear from around the nucleus, and other dark ones replace them; these Dr. Major is disposed to regard as fatty, but he has not succeeded in dissolving them with ether. In other cells, especially in the occipital lobes, the changes were chiefly shrinking and atrophy. Drs.

¹ 'West Riding Asylum Reports,' 1874.

Tuke and Rutherford¹ described a hypertrophied condition of some of the pyramidal cells; but this observation has not been confirmed by Dr. Major or the other observers, although Dr. Major admits the fidelity of Dr. Tuke's description. Dr. Major describes heaps of granules scattered through the brain, which he cannot satisfy himself have in all cases resulted from the degeneration of nerve-cells. Wille² describes heaps of granules and disintegrating blood-corpuscles in the neighbourhood of the blood-vessels; it is possible these may account for some of the appearances seen by Dr. Major.

The Fibres.—Besides the above-mentioned destruction of the cell-processes, the fibres throughout were coarser than normal, twisted, irregular, and in some cases broken up.

The Neuroglia.—The most decided changes were wasting and atrophy; the fine retiform appearance of the normal tissue was lost and patches appeared broken down in an irregular manner, within which nothing but a mass of molecules and nuclei could be detected. In general the nuclei presented their usual characters, but were somewhat increased in number, and collected here and there in small groups. No appearance of proliferation was observed, but in some advanced cases they were shrivelled and deformed.

A careful consideration of these facts leads us to look upon the degeneration of the cells as the essential and characteristic lesion; we believe this is Dr. Major's view, and in his paper on the appearances of the brains of very aged and apparently demented animals³ he describes the cell changes as occurring without the accompanying alterations in the vessels. If Dr. Batty Tuke's view were correct, evidences of excessive senile decay ought to be present throughout the body. We have seen most marked atheroma of the cerebral vessels, even those of the brain-substance itself, which stuck out like bristles from the surface of a section, but the granular degeneration of the cells was not advanced, and the brain was that of a sane although aged person. We are fully inclined to give chronic vascular changes their full share in the morbid process, but it seems necessary to have either a special predisposition or a powerful exciting cause operating upon the nerve-cells themselves, in order to make up the efficient factors of the disease.

We should like to know the condition of the kidneys in Dr. Wille's cases; granular kidney so often coincides with chronic arteritis and atheroma, that some of his cases may have

¹ 'Edin. Med. Journal,' 1869.

² Op. cit.

³ 'West Riding Reports,' 1875.

been what Dr. Major, perhaps, would call insanity of Bright's disease.

If we accept Dr. Major's facts we must admit the existence of a definite pathological condition; granular degeneration of the nerve-cells occurring altogether out of proportion to the coexisting changes in the vessels and neuroglia. In general paralysis the prominent lesions were in these last-named elements, and the changes in the cells were secondary to and evidently resulted from the chronic interstitial encephalitis.

The last group of cases which we shall discuss is one which has given rise to much dispute, and lately formed the subject of an article in these columns;¹ we refer to syphilitic insanity. Dr. Wilks² has contended that the cases of insanity due to gummatous deposit cannot rightly be termed syphilitic insanity, because the symptoms are due not to the syphilitic growth but to ordinary pathological changes resulting from its presence as a focus of irritation. Dr. Wille³ describes three pathological conditions; 1, irritation due to anæmia; 2, meningitis and softening; 3, circumscribed inflammatory softening, atheroma of the vessels, and gummatous neoplasm of the brain and meninges. The first is quite hypothetical, and if true is allied to other forms of anæmic or post-febrile insanity, the second, as it stands, presents no specific characters, though such changes might be caused by syphilitic endarteritis; while both it and the last are open to the objection made by Dr. Wilks. It does appear, nevertheless, as admitted by Dr. Hughlings Jackson,⁴ that the term syphilitic insanity is clinically justifiable and necessary; and as any classification is useful in so far as it correlates the clinical and pathological facts, we believe that it is worth while inquiring to what extent pathologists have been successful in establishing distinctive morbid conditions in the brains of the syphilitic insane. Putting aside gummatous deposits without further discussion as to the title they give us to consider syphilitic insanity a pathological entity, we have had two conditions brought forward, additional to those described by Wille.

Dr. John P. Gray⁵ publishes a case of syphilitic insanity in which the only specific lesion was the "fatty degeneration observable in constitutional syphilis," which affected the nervous system as well as the other organs of the body. Fatty degeneration is such a universal change in all marasmic conditions that we cannot consider this an addition of any value; it is altogether too general a condition for our purpose.

¹ 'Brit. and For. Med. Chir. Rev.,' Oct., 1876.

² 'Journal of Mental Science,' April, 1874.

³ 'Allgemeine Zeitschrift für Psychiatric,' 1872.

⁴ 'Journal of Mental Science,' July, 1875, p. 207.

⁵ 'American Journal of Insanity,' July, 1874.

The other observations are a case published by Dr. Batty Tuke¹ and one by Dr. J. J. Brown.² Dr. Tuke's paper is of importance; omitting the clinical part and assuming for the sake of argument that it was a typical case of syphilitic insanity, we give the post-mortem appearances almost verbatim.

"The dura mater was adherent and the arachnoid was slightly opalescent; small local atrophies existed in the neighbourhood of the intra-parietal fissure. There was an apoplectic clot about the size of a walnut in the centre of the occipital lobe of the right hemisphere; below its level and on a level with the corpus striatum of the same side an apoplectic clot was found, measuring from five to six inches in length, and about one and a quarter inches in breadth at its widest part, extending from about an inch from the tip of the frontal lobe to about the same distance from that of the occipital lobe, bounded on the left by the motor tract, which was not implicated. The two clots were unconnected. In the left hemisphere a yellow softening, irregularly round in shape, and about the size of a large walnut, was found impinging on the corpus striatum, involving the extra-ventricular nucleus and claustrum, and extending to within a few lines of the grey matter of the convolutions. The external arteries were much thickened; on the basilar artery large deposits of a yellowish colour existed. The middle cerebral artery in the fissure of Sylvius on both sides was seen to be nodulated and rendered moniliform by this deposit. Microscopic examination of recent specimens showed thickening of the vascular coats on which irregular swellings were seen consisting of molecular matter. In the grey matter cells undergoing fuscous degeneration were observed in large numbers. In prepared sections the muscular coats of the arteries in the frontal lobes were thickened and the capillary walls were well defined; in the ascending parietal convolutions, transverse sections of the arteries showed the muscular and outer coats much thickened; surrounding the latter coat were concentric rings of a material in which were held corpora amylacea; in some instances empty spaces existed between this material and the brain substance, in others this interspace was filled with a colloid-looking substance; in many of the smaller arteries complete occlusion had taken place. Fuscous degeneration of the cells of the fourth and six layers was observed in degrees between simple and slight deposit and complete destruction. In the occipital lobes and cerebellum the same lesions were noted in a slighter degree. In the corpora striata and in the cerebral convolutions most contiguous to them the diseased condition was more thoroughly marked than in any other region. In some instances the muscular coat was found at least four times thicker than normal, and the connective rings of new material extended from the $\frac{1}{80}$ th to

¹ 'Journal of Mental Science,' Oct., 1876.

² Ibid., July, 1875.

the $\frac{1}{150}$ th of an inch around the smaller arteries. The smaller vessels were very generally completely occluded. Immense deposits of hæmatoidin were found immediately below the ependyma ventriculorum and in the perivascular canals." He goes on to describe the lesions in the pons, medulla, and cord, in which there was nothing special except that the lesions were symmetrical. "No evidence of syphilitic deposit was found in any other organ."

After referring to the general correspondence of the post-mortem appearances with the symptoms observed during life, Dr. Tuke says—

"Microscopic dissection showed that the seat of the most marked morbid changes was in the neighbourhood of the left corpus striatum, producing a softening, implicating its extra-ventricular nucleus and claustrum."

And again he says—

"The specimens on the table show that heterogeneous disease of the vessels is best marked in the immediate neighbourhood of the diseased tracts."

He assumes that the affection of the vessels was primary and also that it was syphilitic, referring to the descriptions of Oedmansson and Fränkel in support of the latter view; he is willing to concede, if need be, that all previously described affections of the nervous system scarcely justify the term syphilitic insanity.

The real question is whether this affection described by Dr. Tuke is syphilitic or whether it is not. Since Dr. Tuke published his case the researches of Heubner¹ have become known in this country, and we are now familiarised with the microscopical appearances of a primary syphilitic endo-arteritis which attacks the medium-sized and smaller vessels, and consists in the formation of circumscribed tumours growing chiefly by proliferation of the elements of the intima, projecting into the lumen of the vessel, tending to undergo organization and leading frequently to thrombosis. It might have been possible that the nodules described by Dr. Tuke as occurring on the basilar and middle cerebral arteries were instances of this affection, but he says that they consisted of molecular matter, which strongly inclines us to believe that they were atheromatous. It is, however, to the changes in the small vessels near the patch of yellow softening that Dr. Tuke especially directs our attention as instances of syphilitic arteritis. In the 'Centralblatt'² for 1876 Friedländer describes an extremely widespread affection of the arterial system, which, he says, hitherto had remained almost unknown.³ He says—

¹ 'Die Suetische Erkrankung der Hirnarterien,' Leipzig, 1874.

² 'Centralblatt für die Medicinischen Wissenschaften,' Jan. 22, 1876. 'London, Med. Record,' 1876, p. 58.

³ It is described by Cornil and Ranvier, 'Manuel d'Histologie Pathologique.' Paris, 1873.

"I refer to the development of a connective tissue, very rich in cells, within the inner coat of the middle and smaller arteries, which leads to a narrowing of the lumen, and at last to a complete filling of the lumen with firm material and obliteration. The affection begins in the acute stage with a proliferation of small thickly crowded round cells, between the inner elastic lamina and the epithelioid lining. The cells afterwards increase in size, a larger or smaller quantity of intercellular substance appears, the tissue acquires the character of granulation tissue, or even of mucous tissue (there being, however, no mucin reaction); new vessels are also observed, often very small arteries, proved beyond doubt to be such by the presence of circular muscular fibres. The proliferation either takes place at all points of the periphery simultaneously, so that a gradual constriction of the lumen is produced, or with greater intensity on one side than on another. In the latter case a transverse section has the form of a meniscus. The lumen is then narrowed on one side, and has an eccentric, often quite peripheral position. The character of granulation tissue is either retained by the proliferated layer for a long time, or it passes into a finer and, in some circumstances, perfectly sclerotic connective tissue, such as is found in firm fibromata."

Further on he says—

"It may be laid down as a general law, that the wandering elements of the arteries also, on their part, take a considerable part in the great group of interstitial processes. According as acute inflammation (collective of small round cells), formation of granulation tissue (in chronic inflammation), induration or caseation, takes place in the interstitial tissue supporting the vessels, the corresponding processes take place in the arterial wall itself, especially in the inner coat."

He alludes to similar changes having been observed by him in most neoplasia, and in fact the sum of his observations is that wherever hyperplasia is going on in the tissues the arterial walls, especially the intima, share in that change. We have paid considerable attention to this subject, and have succeeded in verifying Friedländer in a large number of cases, and we are in a position to state, from our own actual observations, that the arteries described and figured by Dr. Batty Tuke do not present any specific anatomical characters of syphilis, but resemble those we have found in chronic inflammatory tissues of non-syphilitic nature. We have communicated these views to Dr. Tuke and have forwarded him a drawing of an artery taken from a simple polypus of the uterus, which is strikingly like the central figure in his plate. We would refer especially to the stress laid by Dr. Tuke upon the occurrence of these changes in the neighbourhood of the softening, and our view is that the necrosed area, originally resulting from rupture or plug-

ging of some of the diseased terminal vessels, branches of the middle cerebral, distributed to the corpus striatum (see Heubner and Duret's descriptions), formed a focus of irritation leading to chronic inflammatory changes in the adjacent brain substance which were shared in by the coats of the arteries. The same criticisms apply equally to Dr. Brown's case, in which the changes were consecutive to an old-standing hæmorrhage into the pons; the thickening of the coats was much less marked, and the molecular matter around the vessels looks to us like the remains of old extravasation; it is, however, quite possible, or even probable, that the original disease which led to the hæmorrhage may have been syphilitic, but we contend that this has not been shown.

In Dr. Brown's case the age of the patient at the time of the hæmorrhage (20), is much in favour of the primary disease having been syphilitic; in Dr. Tuke's there is considerable doubt from the probability of the disease of the larger vessels being atheromatous; but in both cases the authors believed they had discovered a general disease of the cerebral vessels productive of insanity, and of this we contend there is great want, if not entire absence, of evidence. On these grounds we must contend that these attempts to establish syphilitic insanity as a pathological entity have failed.

We cannot conclude without expressing our feeling that, so far as the work has extended, the results are very encouraging; more labourers are wanted, more reports of special cases with accurate post-mortems and microscopical examinations of the brain, and we would add that the reports of the post-mortems should give information as to the conditions of other organs. We do not know enough of the general histological changes in what are looked upon as local affections; we want to know whether other organs share to any extent with the nervous system in the pathological changes. Every carefully recorded case is an addition to our knowledge, and we hope to see more productions from histologists like Dr. Brown; his two cases are very ably recorded, and apart from the question of syphilis are valuable, and we must not forget that the criticisms we have made are based upon observations which were at the time Dr. Brown wrote scarcely known in this country. Dr. Tuke has done so much for the morbid anatomy of insanity that we can only hope his labours have not ended. Pathological knowledge must be slowly built up; we gladly welcome all additions to it. At present the morbid anatomy of the brain is in a very fair degree of development; but the problems of pathology in correlating lesions with clinical phenomena and in explaining the ætiology of morbid processes depend upon physiology and general pathology, which are at present still far too imperfect to permit of any trustworthy synthesis. We are of opinion that all elaborate

classifications should be postponed, and that, working on the old lines, individual cases should be examined and reported upon until a sufficient basis of material is collected.

VIII.—Thompson and Harris on Lithotomy and Diseases of the Urinary Organs.¹

THE well-known clinical lectures by Sir Henry Thompson have now reached their fourth edition. The present volume is somewhat increased in size, and contains a good deal not to be found in former editions, but essentially the lectures remain the same; the same arrangement is maintained, and they are in the same easy colloquial style which has always made them such pleasant reading. After some preliminary matter the author passes on to the consideration of stricture of the urethra and its treatment, a subject about which more difference of opinion may be said to exist at the present time than at any previous period. During the last few years, both on the Continent and in America, there has arisen a tendency to reduce the treatment of stricture as much as possible to a merely mechanical proceeding, in which, after a guide has once been obtained to the bladder, an ingeniously contrived machine may be trusted to complete the cure. This mode of treatment, perhaps, reaches its highest development in the hands of Dr. Otis, of New York. According to him, a certain relation exists between the circumference of the penis and the calibre of the urethra; and having ascertained the former, he cuts the urethra up to the size he considers the right one, taking no consideration of the varying diameter of the normal urethra at its various parts. The urethrotome cuts everything alike that resists it, whether it be a pathological stricture or a normal narrowing of the canal. In the same way the system of forcible dilatation advocated by Holt splits or stretches the urethra equally through its whole extent. In defence of these mechanical modes of treatment it is alleged that, from the soft and yielding nature of the structure of the normal urethra, only such parts as are abnormally indurated or narrowed can suffer, while the healthy textures escape, and there is no doubt that this is more or less true of forcible dilatation. The same cannot, however, be said of internal urethrotomy, as performed by Dr. Otis. If it be attempted to cut a urethra

¹ 1. *Clinical Lectures on Diseases of the Urinary Organs.* By Sir HENRY THOMPSON. London, 1876.

2. *Lithotomy and Extraction of Stone from the Bladder, Urethra, and Prostate of the Male, and from the Bladder of the Female.* By W. POULETT HARRIS, M.D. London, 1876.

up to one uniform size, and that the maximum diameter of its healthy parts, it is evident that many other parts that are not really strictured must suffer in the operation. Against these purely mechanical modes of treatment Sir Henry Thompson protests most strongly. He points out with great force that, as in every other disease, the mildest means of treatment consistent with efficiency should always be employed, and that to cut or forcibly rupture every stricture indiscriminately would be as wrong and as unscientific as it would be to amputate every diseased knee-joint without first trying what may be done by milder means. In the treatment of stricture, therefore, the surgeon should never, if he can help it, go beyond simple dilatation. This, Sir Henry Thompson asserts, acts merely mechanically, by "stretching the organized lymph, which forms bands round the canal at the strictured point." He denies that it in any way produces absorption of this tissue, as is usually supposed; yet it seems to us that it must be within the experience of most surgeons that, after one passage of a catheter, a larger instrument will often pass in a few days without any appreciable resistance. Moreover, if the process is purely mechanical, why not stretch the urethra up to the full size at once, instead of spending days or perhaps weeks in the gradual process of dilatation? In this mode of treatment Sir Henry Thompson always prefers soft to solid instruments, and his opinion is the more valuable as he tells us himself that he has completely changed his opinions since he first wrote on this subject. In the later stages, however, he strongly recommends the employment of polished conical steel bougies, varying about two numbers of the English scale from the point to the thickest part. These views will no doubt meet with the approval of almost every practical surgeon at the present time; and doubtless a vast amount of human misery would be saved if the French bulbous bougies and catheters formed a part of every surgeon's stock of instruments. At the present time it is still too common, especially in the country, to have recourse in every case to solid instruments.

If simple dilatation fail, the next resource is continuous dilatation by tying a soft catheter into the urethra. There is no doubt this treatment is marvellously rapid and usually perfectly innocuous. A urethra which at first only admitted a No. $\frac{1}{2}$ may in a few days take a No. 12 with ease. The troubles usually met with arise from the cystitis that is so often set up by the presence of the catheter in the bladder. Moreover, the catheter opens up the bladder, as it were, to the air, and so favours decomposition of the urine, which infallibly aggravates any existing cystitis. Now, cystitis is not a thing to be lightly set up, as it is not always easily allayed again, and the tendency to its occurrence has always been considered a drawback to this mode of treatment. In order to prevent it

Sir Henry Thompson recommends the catheter should be tied in, in such a way that the eye is only just inside the bladder, and be accurately fixed in this position by being fastened to the penis itself and cut off to the proper length. If this simple precaution be taken, Sir Henry Thompson states that cystitis seldom gives rise to any trouble, and after eight to ten days simple may be substituted for continuous dilatation. But excellent as these modes of treatment doubtless are, they frequently must fail on account of the resilience of the stricture, never on account of mere tightness. All strictures in front of the scrotum and many in the bulb show this character of resilience in a marked degree. Now, in these cases Sir Henry Thompson's next resource is internal urethrotomy, and for this operation he prefers Civiale's urethrotome, cutting from behind forwards, to any other, except, of course, for the orifice or first inch, in which parts a small bistouri caché serves every purpose. Before attempting internal urethrotomy he would always dilate the urethra by continuous dilatation to about No. 6, a size which just admits of the passage of the urethrotome. He prefers this mode of operating to any more purely mechanical means, because he believes it can be intelligently directed by the surgeon. On this subject he writes as follows :

"Now, suppose for an instant that it were possible for you to have the stricture before you open to your eyes and hands ; you certainly would take a little scalpel and employ it for each case according to its needs, intelligently dividing the obstructing fibres as much as is necessary, and no more. But with very little practice it is perfectly easy to do this without seeing the stricture—that is, to apply a little scalpel, dividing where you wish, and not elsewhere, just as you divide the constricting fibres in the ring of a hernial sac. And I need scarcely say, that this intelligent action of yours will produce something better than the work of a machine, which any ignorant person can employ just as well as you. The difference in the two cases resembles that which exists between the music of an organ produced by a handle and the music which results from the facile hand of a skilful performer."

This is, no doubt, absolutely true, and no instrument, however ingeniously contrived, can possibly act with as much precision as the skilful hand of a surgeon. There are, however, other instruments besides Civiale's which may be made to cut with almost equal precision ; for instance, Mr. Berkeley Hill's urethrotome, in which there is a most ingenious combination of stretching and cutting. A wedge containing a small sheathed knife is made to pass down between two blades, separating them, as in Perrève's dilator. No force is used, but as soon as the wedge is felt to hitch against the stricture the concealed knife is protruded, and, cutting from before backwards, notches the stricture, which is kept steady and slightly

stretched by the separated blades of the dilator. By this instrument the strictured portions only are cut, and, supposing it to be of moderate size, the cutting can be done with as much certainty and be as accurately limited to the stricture as if Civiale's instrument were used. Great stress is laid upon the complete division of the stricture, and Sir Henry Thompson tells us that we need not fear freedom of incision. He has never seen any harm produced by it, and has "never had to regret a free and complete division."

These, then, are the great principles of treatment laid down in these lectures. Always, whenever possible, use soft in preference to solid instruments. Treat every stricture by simple dilatation if you can. If that is not successful, or if there is great difficulty in the first introduction, employ continuous dilatation followed by simple dilatation; and if the stricture refuses to yield to these means, do internal urethrotomy, using some instrument by which the incision can be regulated accurately by the hand of the surgeon. Extraordinary means, such as external urethrotomy, puncture of the bladder, &c., can only be required in rare and exceptional cases. Auxiliary means, such as rest in bed, diet, and attention to the bowels, form valuable adjuncts to the treatment, but Sir Henry Thompson does not seem to expect so much from them as some surgeons. As to permanent cure, claimed by so many writers, past and present, for their own particular mode of treating stricture, Sir Henry Thompson does not believe in it. "The treatment has yet to be devised," he says, "which will remove absolutely and for ever the occurrence of recontraction in a patient once the victim of an organic stricture."

Enlargement of the prostate, prostatitis, retention of urine, and vesical tumours, are treated of with admirable clearness, but space prevents our noticing the many interesting points raised in their discussion, and we must confine the rest of this notice to the lectures on stone in the bladder and its treatment.

Sir Henry Thompson treats of this subject from its very beginning, one lecture being devoted to the early history of calculous disease and the treatment best adapted for its prevention. Excluding phosphatic calculi, which are, of course, always of bladder origin, and can only be prevented by local means calculated to prevent decomposition of the urine and consequent cystitis, nineteen out of twenty calculi are composed of uric acid, and the preventive treatment recommended is consequently that usually adopted in the gouty diathesis; but Sir Henry Thompson differs from many in his absolute condemnation of alkalies, including Vichy water, in the treatment of this condition. On this subject he says—

"Now, it is quite true that with alkalies, provided enough be taken, these deposits (urates and uric acid) will disappear; the uric

acid will no longer be deposited; the urine will become less irritating; the annoying symptoms will be diminished or got rid of; and, of course, the patient is very much pleased with this new condition of clear urine and disappearance of all deposit. And you will say, 'What more can be desired?' This: you have merely made his enemy disappear, but he is by no means rid of its presence.

"You have not checked the acid formation. The uric acid is there as much as ever; but the uric acid and the urates are soluble in alkali, and you have only made them invisible. You really have the same condition as that of the fabled ostrich, which is said to put its head in the bush when pursued by hunters, and, no longer seeing them, to believe itself secure."

Not only this, but Sir Henry Thompson believes that large quantities of alkali habitually taken act injuriously, and tend possibly to increase the tendency they are given to check. It is upon attention to diet and to the employment of "such agents as will stimulate the excretory action by the *primæ viæ* without depressing vital power," that, Sir Henry Thompson relies in the treatment of the uric-acid diathesis. The best agents of this kind he believes to be the various purgative or aperient mineral waters, such as Friedrickshalle, Püllna, Carlsbad, and Hunyadi János, and, instead of sending a patient suffering from gravel to Vals or Vichy, he would recommend a course at Carlsbad. But, doubtless, Sir Henry Thompson is conferring a great benefit upon patients suffering from these affections when he points out that a course of these waters, if taken at home, combined with judicious diet and abundant fresh air and exercise, will do as much good as if taken on the spot, and probably more than if drunk in the enormous quantities usually recommended at the watering-places. It is an interesting fact which Sir Henry Thompson states, that he has verified by experiment that these waters possess more active powers than artificial solutions of similar chemical composition. He says—

"If you will reduce by careful evaporation, as I have done, such mineral waters to their pharmaceutical condition of crystallised salts, you will find them possessing little if any more power than similar salts as obtained by the ordinary processes, and met with in every chemist's shop. They no longer do their work on the same terms as when administered in the original water before they were separated by evaporation."

Supposing the preventive treatment to fail, the next point is early diagnosis of the stone. In the present day it seems almost a disgrace to our profession that stones should ever reach a large size, and yet how frequently it happens still that patients are allowed to go on for many months, or even for years, without being sounded. In every case of frequency of micturition, especially in patients past middle life, and perhaps of a gouty tendency, stone should be the

first thought of the surgeon, not, as is too frequently, the last, and only arising when months of medical treatment have proved abortive and the symptoms have gradually but steadily increased. As Sir Henry Thompson points out, the pain after passing water is in the early stages "a mere passing sting." If even this amount of pain occur, with frequency of micturition greater by day than by night, and possibly a minute drop of blood after riding on horseback or in a carriage, the patient should be sounded without delay. Sir Henry Thompson, moreover, calls attention to the fact that stones sometimes get overlooked for a time, the patients getting into the hands of some one whom the ordinary etiquette of the profession forbids to use a sound or catheter, where they remain under treatment for gouty cystitis, catarrh of the bladder, and such like, till, their symptoms becoming more severe, a surgeon is called in.

The author recommends that the physician should use a sound; we think, on the other hand, it would be much better if all cases in which the bladder is evidently the seat of the disease he should at once hand the patient over to the surgeon, for, taking bladder diseases as a whole, internal remedies play but a small part in their cure.

A very interesting lecture in Sir Henry Thompson's work is devoted to the history and prospects of the solvent treatment of calculus. On this subject the only experiments of any value are those of Dr. Roberts, of Manchester, and it may be said that he has conclusively shown that there is no hope of any success except in dealing with very small uric-acid calculi, not coated with phosphates, and even with these the treatment is slow and doubtful, no recorded case ever having proved successful. Such stones can be certainly removed by one or two applications of the lithotrite, and the patient rid of his trouble in a few days; and the solvent treatment should, therefore, we think, be finally abandoned as not only useless, but dangerous, as every day a stone lies in a patient's bladder the difficulties and dangers of the cure are increased.

The operation of lithotritry has now been before the profession in a practical form for over fifty years, and we believe it may be said without fear of contradiction that it has reached its limit of perfection. The instruments employed have been reduced to the minimum size consistent with safety; they are so designed that it is almost impossible for the coats of the bladder to be seized between the blades, and the mechanism by which the crushing force is applied to the stone is so simple that any improvement is scarcely conceivable. The only question before the profession at the present time is the limits of its applicability, and the vast experience of Sir Henry Thompson, which has been at various times laid before the profession, goes far towards settling this point also. His views on the subject he summarises by saying "in general terms, that all the cases above puberty are to be crushed, with certain exceptions."

These exceptions are, an oxalate of lime calculus over one inch in diameter, and a uric acid or phosphatic calculus two inches or more in its long diameter. Stricture of the urethra, enlarged prostate, atony of the bladder, or irritability and extreme sensibility of the bladder, are not in Sir Henry Thompson's opinion sufficient reasons for abandoning the operation and falling back on lithotomy. Stricture of the urethra he dilates till the lithotrite can be introduced; in enlarged prostate the stone can be reached to the last fragment by reversing the blades of the lithotrite; in atony and in enlarged prostate the fragments may be removed by Clover's aspirator, and in extreme irritability of the bladder the condition will be relieved by crushing the stone. Thus it will be seen that increasing experience has led the author rather to an extension of the operation to cases formerly considered unfit than to a limitation of it within narrower bounds. The size of the stone seems to be almost the only counter indication recognised by Sir Henry Thompson, and, as medical science and education advance, stones of the sizes above mentioned should be almost unknown, and accordingly he says, "I hope you will live to see the day when lithotomy for adults will be one of the rarest operations in surgery. I do not suppose I shall; but I do expect to live to see one thing, and that is, lithotomy becoming less common year by year." These, then, may be taken as the views of the greatest living advocate of lithotomy; on the other hand, many surgeons of eminence still believe that it is better practice to cut for every stone, however small. They urge against lithotrity, that although the patient can be easily rid of his stone, he is usually left with an almost incurable condition of chronic cystitis, with a tendency to decomposition of the urine and consequent formation of secondary phosphatic calculi. There is no doubt this accusation is in part true, but in reply it is urged that when this unfortunate result occurs it is not in cases in which before the operation the bladder was tolerably healthy and the stone small, but in old subjects with much previous bladder mischief, and in whom perhaps the stone was originally phosphatic, and merely secondary to the bladder condition. Now, these cases are markedly unfavorable for the cutting operation, and the advocates of lithotrity say it is better for a man to live with cystitis than to die from lithotomy. Some statistics published last year by Sir Henry Thompson certainly support strongly the view that lithotrity for small stones in a healthy bladder is infinitely safer than any cutting operation and by no means frequently followed by any after-troubles; in fact, he states that he has never yet lost a patient of any age in whom the stone was not larger than a common nut, and with such results as these we may fairly set off a few cases of persistent chronic cystitis against the deaths that would almost certainly have occurred had lithotomy been performed. We do not think that with the evidence

at present before the profession any one would be justified in cutting a patient for a small stone except in very exceptional circumstances. With regard to larger stones, more evidence is still required before the limits of lithotripsy can be determined, but few surgeons would go so far as Sir Henry Thompson and attack a stone of two inches in diameter even if it were known to be phosphatic, and Sir Henry himself limits the size of oxalate of lime calculi to one inch. The diagnosis of the exact nature of the stone is not always practicable even in the most skilled hands, and most operators would doubtless prefer the smaller limit for every variety of stone. Cases complicated by enlarged prostate and atony of the bladder will probably always remain in the debatable ground between the two operations, each surgeon following his own opinion with regard to them.

When lithotripsy terminates fatally, it is in the vast majority of cases due to acute suppurative inflammation of the kidney, and it is to the study of the causes, exact nature, and prevention of this accident, that we must look for the future improvement of the results of this operation. Two points, however, are tolerably certain with regard to it—first, that it arises in the great majority of cases in kidneys previously the seat of chronic disease, secondary to the affection of the bladder; and secondly, that the presence of decomposing urine in the bladder, ureters, and pelvis of the kidney, acts on the determining cause of the final catastrophe. It is of the greatest importance, therefore, to be able to recognise beforehand the condition of the kidney; but this task is by no means so easy as it would at first sight appear to be. As the disease of the kidney is interstitial and not catarrhal, it gives rise to but little albumen or renal derivatives of any kind, and the examination of the urine is always rendered difficult by the presence of blood, pus, and mucus, of bladder origin. Further investigation may some day enable us accurately to ascertain the state of the kidney, but at present it must be confessed it often baffles our most careful search. The second cause is, no doubt, more under our control, and we think there is an unreasonable prejudice in the minds of many surgeons against washing out the bladder during the progress of the operation of lithotripsy. However irritating the process may be, it can scarcely be more so than the presence of putrid pus, mucus, and urine in the already inflamed bladder. When serious disease of the kidneys is known to exist the question naturally arises, Should lithotripsy or lithotomy be performed? Sir Henry Thompson unhesitatingly decides in favour of lithotripsy, although in this he is certainly opposed to the majority of surgeons. There is no doubt it is a difficult question to decide; nothing, as is well known, has so fatal an influence on any cutting operation in any part of the body as renal disease; and, on the other hand, we have to consider the increase of the existing cystitis, so likely to follow the operation of

lithotrity, and the dangers of decomposition and inflammation extending to the ureters and kidneys, and so giving rise to suppurative nephritis. Sir Henry mentions six cases in which he performed lithotrity, of which he says, "To have placed these feeble and pallid frames on an operating table for lithotomy would have been to slaughter them outright. Of these six, five were saved." This is strong evidence, and in the face of it we must acknowledge that further investigation is required before the question can be considered as definitely settled.

With regard to lithotomy we have but little to say. There is no doubt that the operation reached its perfection long ago, and that human ingenuity is not capable of inventing anything really new about it. Mr. Harris has, in his work on lithotomy, carefully collected the views of all the more important modern authors concerning the operation, and we only regret that he has not added a little more of his own, as his great experience, extending to 365 cases, fully entitles him to do. There is nothing new in the book, nor is it possible that there should be. Two centuries of fierce discussion have left the subject nearly threadbare. The only point in which we would suggest an alteration is in the anatomy. Neither in Sir Henry Thompson's work nor in Mr. Harris's do we find any mention of the lateral true ligaments of the bladder. Now, the attachment of these ligaments is, we believe, the most important point in the deeper anatomy of lithotomy. Beyond the fibrous diaphragm, formed by the recto-vesical fascia of which they are part, we have merely loose pelvic cellular tissue, continuous with the subperitoneal fat, a tissue of such looseness of structure that extravasation of urine occurs in it with the greatest possible facility; in front of it we have tissues much more firmly held together by connective tissue, the levator ani, ischio-rectal fat, &c., which offer considerable obstruction to any infiltration of urine, and amongst which, even if it occurs, it will not of necessity give rise to fatal mischief. The incision may therefore be carried back with perfect safety to the attachment of this ligament, the capsule of the prostate being absolutely ignored. If, however, it be carried beyond this point, the loose pelvic cellular tissue is opened, and infiltration of urine is extremely likely to occur. The possibility of performing lateral lithotomy for a stone of any size, without cutting or tearing the capsule of the prostate, has long been denied by Professor Ellis, and more lately by Mr. Teevan; and as it was the fear of cutting this that led to the limited incisions and the so-called dilatation, or more properly speaking tearing, of the prostate, it is evident that a more correct view of the anatomy would do much to encourage those who believe a stone should be cut out and not torn from the bladder.

In the operation of lithotomy, as recommended by John Bell and

as it is said to have been performed by Cheselden in his later years, the incision is carried completely through the capsule of the prostate and as far as the attachment of the lateral ligament to the bladder. It will be remembered that the ligament crosses the side of the prostate, obliquely leaving that gland a little below the middle of its posterior part, and then passes on obliquely over the neck of the bladder, so that small part of the anterior portion of the trigone is situated in front of the ligament. John Bell and Cheselden operated by carefully dissecting between the prostate and rectum till the staff could be felt at this point, covered only by the tissues of the bladder. The knife was then thrust in immediately behind the prostate, and carried forwards to the membranous portion of the urethra, dividing the whole gland with its capsule. The whole incision was in front of the lateral ligament; the largest incision had been made that was consistent with safety, and if after that violence was done to the neck of the bladder, or the tissues rent backwards beyond the recto-vesical fascia, it was no fault of the surgeon. This seems to us to be the perfection of lithotomy, in which nothing is left to purely mechanical means, but exact landmarks are found and the incision made in such a manner that the surgeon knows exactly what he is doing and how much he has cut. John Bell speaks of the operation as one any surgeon ought to be able to do, but we cannot help thinking it must have been much more difficult than he pretends, and we should hardly think that it will ever be revived in practice, however perfect it may be in theory.

Before concluding we must once more record our admiration of these lectures by Sir Henry Thompson. They contain condensed in a very small space and in a most readable form all that the practitioner need know of the surgical diseases of the urinary organs. There is no class of diseases in which more egregious blunders are made or in which an error of diagnosis may cause more misery to the patient or disgrace to the surgeon, but we feel sure that such mistakes will hardly occur to any one who reads and remembers the details of diagnosis and treatment laid down in these lectures.

IX.—Cormack's Clinical Studies.¹

As the title of Sir John Cormack's work implies, the author presents in these two volumes a series of practical dissertations on a great number of subjects, founded upon clinical experience and

¹ *Clinical Studies, illustrated by Cases observed in Hospital and Private Practice.* By Sir JOHN ROSE CORMACK, K.B., M.D. Edin. and Paris, Physician to the Hertford British Hospital of Paris. In two volumes. London and Paris, 1876.

arranged without any special system. The materials are drawn from the wards of the Edinburgh Fever Hospital, from private practice in and about London, from the battle-grounds in and around Paris during the German siege and the reign of the Commune, from the wards of the Hertford Hospital in Paris, and from private practice in the same city. Many of the articles have appeared in the form of separate monographs and papers published in contemporary medical journals, and some of them date from an early period of the author's career, but they form altogether a valuable repository of facts carefully collected during a lifetime devoted to the pursuit of Medicine in its widest sense, for the subjects are drawn indiscriminately from the domains of Medicine proper, Surgery and Midwifery.

The first article in Sir John Cormack's volumes consists of the reprint of a treatise on the epidemic fever which prevailed in Edinburgh and other places in the year 1843, the treatise in question having been printed and published in that year. As is very well known, the ideas relating to the epidemic fevers have very much changed in recent years, and doctrines which were formerly held by the profession at and before the time of the publication of Sir John Cormack's treatise are no longer tenable. With respect to the particular fever described, it is now regarded as having been an epidemic of relapsing fever, and indeed is so named in the present volume by Sir John Cormack himself. The mysterious appearances and disappearances of relapsing fever form a series of the most interesting but at the same time most obscure problems in the history of epidemic diseases. Very little known in the present day, and occurring but seldom in England, it would seem, from the researches of Dr. Murchison, that this form of fever has occurred at fitful and irregular and sometimes distant intervals of time in different countries; and, moreover, that the sthenic or inflammatory fever, the synocha of our ancestors, was no other than this disease. So extensive was the visitation of this fever in Scotland at the date now referred to (1843) that the total number of cases in Glasgow was estimated at 33,000, or $11\frac{1}{2}$ per cent. of the entire population, and in Edinburgh at 9000.

Sir John Cormack, while fully admitting that his knowledge of fever is now much greater than it was when his treatise was written, yet considers that there is an obvious advantage in reprinting the work without the admixture of any new matter and without any important alterations. It constitutes, in fact, the history of a particular epidemic, and the author claims for it only the merit of being a faithful clinical record of observed facts, the reports being invariably committed to writing at the bedside, and the accounts of the post-mortem examinations being all written while the appearances were being actually observed. Under these circumstances, the treatise must be taken only for what it is represented to be, and must by no means be regarded as a contribution to the pathology.

of fever as it is observed in the present day. Many of the plans of treatment, also, have been either abandoned or very considerably modified since 1843; bleeding and purging, for instance, having been proved to be useless, if not mischievous, and specific therapeutic measures being of questionable utility. It is only just to Sir John Cormack to state that his remarks throughout prove him to have been an attentive and careful observer, and the record of symptoms, results, and treatment given in his pages strongly confirms the opinion that the fever was really the Relapsing Fever, and was neither Typhus nor Typhoid.

The subject of Cholera occupies nearly a hundred pages of Sir John Cormack's first volume, but the writer did not apparently enjoy so many opportunities for the practical examination of this disease as for that of the Edinburgh fever epidemic. His own experience of cholera was mainly derived from cases observed at Putney, near London, where he was residing at the period of the visitation of the malady in the metropolis in 1848-49. One of his own children, however, was seized with the disease in a very aggravated form, but eventually recovered, and all his other cases were successful, except one, an infant of two months. Hence his personal acquaintance with cholera was but limited, although he compensates for this deficiency of actual observation by pretty copious references to the works and opinions of other authorities. He admits, at the outset of his remarks, that notwithstanding all that has been written on this subject, sufficient data do not yet exist to entitle any one to feel quite confident that his own opinions are built upon an unassailable foundation, and this modest admission will no doubt be echoed by those who have studied the disease with the deepest attention.

He believes, although he does not very strongly insist upon this belief, that there is no great difference between the English cholera and that which is known as the Asiatic, and he adduces two cases in which some of the worst and most characteristic features of the latter affection were ingrafted upon or intermingled with the former, both cases, however, ending in recovery. Probably the strongest argument which can be brought forward against this view is the fact that while English cholera is an annual visitor every summer, the Asiatic form occurs only at rare intervals, whereas if the two were really identical we should have both constantly among us; and moreover, some of the worst visitations of the Asiatic disease have occurred in the winter, when no cases of English cholera or other mild bowel disorders are prevailing. The most reasonable explanation of such cases as those two recorded by the author is that during the prevalence of the Asiatic malady even the mild forms of disease assumed some of the severe features of the malignant one.

His opinions as to the analogy of cholera with fever will probably

meet with more general acceptance, and few will be disposed to question his conclusion that the stage of cholera collapse ought to be considered as an aggravated cold stage of the paroxysm of a pernicious fever, which may spontaneously terminate in death or reaction. No one can have observed the general aspect of the worst cases of cholera, when the disease has come on rapidly, without seeing the resemblance between the whole condition of the patient and that of one seized suddenly with typhus in its worst form, or even with malignant smallpox, in both which cases it seems as if the poison of the disease has at once overcome the powers of vitality and the patient is thenceforth little more than a living corpse. If, however, the patient should by any chance survive this period of collapse, then the next stage (called the secondary fever in cholera) begins to manifest itself and a fresh danger is presented together with reviving powers. Nor can much objection be urged against Sir John Cormack's explanation of the main feature of Asiatic cholera, namely, that—

“The least dense portion of the blood has an excessive tendency to exude through the capillaries of the stomach and bowels, and pass from the body by vomit and stool; and the inspissated residual blood, being unable to pass through the small pulmonary vessels, causes congestion of the lungs, and, as speedy consequences of this condition, paralysis of the right side of the heart from over-distension, asphyxia, and other subordinate derangements of the vital actions.”

With regard to the treatment of cholera, it cannot be said that Sir John Cormack throws any new light upon the subject, nor indeed are his remarks altogether consistent with one another; for, while he enumerates and praises several drugs which are said to be efficacious in various stages of the disease, he admits that the very nature of the malady forbids the hope that any therapeutic measures will be necessarily successful. He judiciously observes that recoveries have too often been designated cures, and he admits that even when let alone cholera shows a disposition to terminate in recovery. His meaning here, no doubt, is that the system, having received the poison, endeavours to throw it off, and if this effort be successful a cure will result; but if, as too often happens, the dose of the poison be too strong, death terminates the scene before the work of elimination commences. That he is pretty well convinced of the practical inutility of any or all the methods of cure proposed in cholera appears evident in his chapter entitled “Cholera-Collapse: how some Recoveries take place,” wherein he gives a case of a very dangerous character, occurring in Paris, and where assistance to the *vis medicatrix naturæ* was the only course adopted, the result being the complete recovery of the patient. He also quotes the experience of Netter, a French army-surgeon, who reported the recovery of some very severe cases of cholera by the use

of copious draughts of water, and, on the whole, Sir John Cormack is inclined to approve the recommendations on the subject of the treatment of collapse and imperfect reaction in this disease issued many years since, viz. in 1854, in a report from the London College of Physicians, in the following words :

“A strict observance of the horizontal position, moderate external warmth, stimulating applications to the extremities, region of the heart or epigastrium, and the administration internally of diffusible stimuli in small quantities, with the free use of ice, cold water, and other diluents, appear to constitute the principal part of the treatment as far as it is yet determined.”

The other contents of the first volume are of a miscellaneous description, and comprise cases and discussions on Scarlatinous Nephritis, Puerperal Convulsions, Granular Degeneration of the Kidney, Infantile Remittent Fever. A case is given of Labour complicated with Cauliflower Excrecences of the Uterus; the Value of the Dark Abdominal Line as a Test of Recent Delivery is discussed and finally regarded as doubtful; a curious case is recorded of difficult labour caused by enormous Cystous Kidneys in the mature fetus; and a short paper is reprinted on Hernia of the Uterus.

A large portion of Sir John Cormack's second volume is devoted to the subject of “Air in the Organs of Circulation,” a subject which the author has almost made his own, having published an Inaugural Dissertation bearing this title, in the year 1837, and to which the Gold Medal was awarded by the Medical Faculty of the University of Edinburgh. This dissertation is here reprinted, together with some other contributions on the same subject published at different periods. Two of the most interesting of these latter are—one on a case of death from the entrance of air by a rigid vein in the neck opened accidentally by a seton needle; and the other on the entrance of air by the open mouths of the uterine veins considered as a cause of danger and death after parturition. The last-named subject formed the basis of Sir John Cormack's graduation thesis in Paris in the year 1870.

The question as to the degree of danger likely to result from the introduction of air into the organs of circulation, and especially from wounds in the veins, is a very important one, both in a pathological and therapeutical point of view, and is very ably handled in the prize essay of 1837, wherein it is shown by original experiments that fatal results may or may not ensue from the presence of air in the circulatory system according to the quantity of air introduced and the mode of its introduction. Without entering at length into the details recorded, it may be stated generally that the author arrived at the following conclusions, viz. that the greater the rapidity with which the air is introduced, and the greater the diameter of the tube through which it passes, so much the more

sudden and deleterious is the result ; and that the cause of death in the fatal cases is the inability of the right side of the heart to contract and expel its elastic contents which consist of air and frothy blood. These conclusions, which were mainly drawn from the results of experiments on the lower animals, received a remarkable confirmation in a case occurring under the author's observation thirteen years after his dissertation was published, and in which the same phenomena occurred during life and the same appearances were observed after death as those detailed in the experiments. The case in question was one in which, by an accidental wound of one of the veins of the neck by a seton-needle, air was suddenly drawn into the circulatory organs, asphyxia and convulsions immediately supervened, and death ensued in seven hours. At the post-mortem examination the wounded vein was found to have thickened and rigid walls, with rather capacious calibre, so that the air was allowed suddenly to enter its cavity, and the right auricle of the heart, which was much distended, contained partially coagulated blood, intimately mixed with bubbles of air, and the pulmonary artery and its branches were also filled with blood and air intimately mixed together. The case, it will be observed, was not immediately fatal, as sometimes happens ; and Sir John Cormack observes that if the distension of the right auricle had been a little more complete and rapid, instant death would have been the result, but as it was, the heart partially regained its power, and the fatal termination was due to asphyxia from the obstruction of the passage of blood through the lungs.

Several cases are recorded in which fatal results have ensued from the cause now alluded to during the course of operations, but the termination by death is by no means constant, as many recoveries take place even when air has entered the circulatory system and dangerous symptoms have manifested themselves. On the other hand, many cases of rapid or sudden death, unaccountable by the ordinary modes of explanation, have been attributed erroneously to the entrance of air into the circulatory system, and Sir John Cormack devotes several pages to the discrimination of the cases which may or may not be so explained.

It is a gratifying reflection that the danger of the introduction of air into the veins during operations is, in the present day, very much diminished by the use of anæsthetics, for the accident referred to was necessarily more likely to happen in former times, when the patient was struggling during the agony of pain and was forcibly held down by assistants.

With regard to the therapeutical measures to be adopted in case such an accident should arise, our information is at present not very satisfactory. It would appear, however, from some rather striking experiments made upon the lower animals, that the abstraction of blood affords the most efficacious means of relieving the over-dis-

tended and labouring heart. This plan was, in fact, adopted in the case above referred to, where the vein in the neck had been wounded by the seton-needle, and although the result was fatal, some relief seems to have been afforded by the bleeding, which was practised at the arm. But such a measure must be resorted to with the utmost caution, and more especially must it be clearly established that the symptoms treated are really due to the presence of air in the circulation, for, as Sir John Cormack shows in some of his papers, the danger often depends upon totally different conditions, which might be mistaken, and, if erroneously treated, would be aggravated rather than relieved. More especially are these remarks applicable to the supposed danger in parturition from the entrance of air into the open mouths of the uterine veins, a circumstance which not only may, but actually has been the cause of sudden death. Sir John Cormack, however, is of opinion that this accident is probably a very rare one, and most of his illustrations and observations are rather directed to show that this is not the cause of death in parturient women in several doubtful cases in which this explanation has been given. If a large quantity of air has really entered the circulation through the open mouths of the uterine veins, he thinks that unequivocal evidence of this occurrence will be afforded by listening to the heart, when a churning sound, caused by the admixture of air and blood in its cavities, will be heard. If death does not immediately ensue, asphyxia will supervene, and the rapidity and violence of the symptoms will depend upon the quantity of air which has entered, on its passing up to the heart in one large volume or in divided quantities, on the presence or absence of hæmorrhage, and on the strength of the patient. Two instructive cases are given of cardiac syncope in females, one habitual, whenever the bowels were unloaded at long intervals, and the other occurring suddenly at the sixth month of pregnancy from collapse of the abdominal bulk caused by a rapid discharge of the liquor amnii. In both these cases a successful result ensued from giving support to the abdominal walls by a belt, and they are adduced as a warning against the risk that such cases may be mistaken and treated as due to the entrance of air into the veins.

In connection with this subject of the entrance of air into the veins, we may observe that a French author, Dr. Louis Couty, has lately written a work entitled '*Étude Expérimentale sur l'Entrée de l'Air dans les Veines et les Gas intra-vaseulaires*,'¹ in which he arrives at pretty nearly the same conclusions as Sir John Cormack did in 1837, when the prize essay of the latter was published. Dr. Couty, however, in giving a rather elaborate and otherwise very able summary of the history of his subject, strangely ignores the labours of Sir

¹ Paris, 1875.

John Cormack ; and while he relates the chief points of the discussion which took place at the Académie de Médecine of Paris in the month of December, 1837, he omits to mention that Sir John Cormack's dissertation was published in the previous August. He erroneously fixes the date of Sir John Cormack's first essay in 1850. Dr. Couty's chief therapeutic resource in the case of entrance of air into the veins during an operation is the abstraction of blood, either at the bend of the arm or from the jugular vein.

In the beginning of the Chapter on the "Reflex Convulsions of Infancy" some remarks are made on the treatment of Infantile Convulsions of reflex origin, illustrated by a case in which a child was restored from a state of apparent death by the hypodermic injection of morphia. In this case, which is a curious and interesting one and is related at length, the source of the nervous disturbance appears to have been the presence of a large lumbricoid intestinal worm, which was finally expelled by a dose of *santonin* and *calomel*, and the patient was completely restored to health.

On the much vexed question of the relations existing between "Croup" and "Diphtheria," and on the affections passing under these names, Sir John Cormack contributes some very valuable observations, the whole of this subject, in all its various aspects, occupying more than 200 pages of the second volume. Sir John, having commenced his studies in the medical schools of Edinburgh, was naturally imbued with the views entertained and taught by the followers of Dr. Home, the author of the well-known monograph on Croup ; but he has since had abundant opportunities, not only of studying the doctrines held by the disciples of Bretonneau, but also of examining for himself the features of the *maladies* which, under the general name of "croup," prevail in Paris and other parts of France, as well as in other countries. The result has been that he has become a convert to the opinions first developed by Bretonneau and subsequently adopted and strengthened by Trousseau and others. One great difficulty in treating the whole subject arises, on the one hand, from the different names applied in different countries and by different authors to the same morbid process ; and on the other hand, to the circumstance that some authors treat of several different diseases under the same name. Thus, as Sir John Cormack observes, croup "is a name under which are ranged diseases essentially differing from each other, and it is, indeed, only intelligible when used as the name of a symptom—as the synonym of stridulous breathing" (vol. ii, p. 191). He tells us, from his own early experience, that when a pupil at the Royal Dispensary of Edinburgh he saw many cases of what was called "inflammatory croup," but which he now believes to have been infantile laryngitis, and in which, under the superintendence of his teachers, he adopted bleeding and purging. When recovery took place in bad cases the result was

attributed to the active treatment adopted, a conclusion which Sir John believes to be probably correct. But he was also told by his instructors "that unless active antiphlogistic measures were employed the case would advance to the generally fatal membranous stage, in which caco-plastic false membrane forms in the larynx and trachea, and sometimes descends into the bronchial tubes. "I have now fortunately learned," he continues, "from Bretonneau and Trousseau that 'membranous croup' is not an advanced stage of 'inflammatory croup,' but the characteristic manifestation of diphtheria, a general disease *sui generis*, in which antiphlogistic measures are exceedingly prejudicial" (loc. cit., p. 191).

Accordingly we find in these pages that the subject which would have been headed "Croup" in many systematic works is treated under the different heads of "Infantile Glotto-Laryngeal Spasm, frequently designated Spasmodic, Spurious, or False Croup," and "Pharyngo-laryngo-tracheal Diphtheria." Another section is entitled "Diphtheria is a Disease; Croup is a symptom of Laryngismus Stridulus, Laryngitis Stridulosa, Laryngo-tracheal Diphtheria, and other Affections;" and other chapters are devoted to the subject of "Paralytic Affections in connection with Diphtheria."

In the section on "Infantile Glotto-laryngeal Spasm" Sir John Cormack describes several cases of what was called "croup" by the relatives, but which were instances of spasm of the larynx, and terminated favorably under appropriate treatment. With regard to these spasmodic laryngeal affections, the author explains a difficulty which has arisen as to the meaning of the words "laryngite striduleuse," used by some French authors synonymously with spasmodic or false croup. He shows that the employment of the word "laryngite," implying an inflammatory affection, as synonymous with another expression denoting only a spasm, is very inconvenient, but it is well known that French authors include in their "neuroses" many affections which are really inflammatory; and, indeed, in young children, in consequence of the narrowness of the glottis, even a slight inflammation of the part will often produce spasmodic difficulty of breathing. Nevertheless, we must observe that it is very unfortunate that the French pathologists should confuse (as they certainly do) our "laryngismus stridulus" and our "infantile laryngitis" under the single term "laryngite striduleuse." It is almost needless to remark that in none of the cases here immediately alluded to is there any false membrane in the air-passages.

In the section on "Pharyngo-laryngo-tracheal Diphtheria," however, Sir John Cormack describes the affection known as "croup," or "true croup," and illustrates his remarks by some excellent typical cases. In one there was a distinct appearance of false membrane on the fauces, giving warning of the danger which, in act, actually supervened and demanded the operation of tracheotomy

to relieve the urgent symptoms of suffocation. The operation was performed by Dr. Jules Simon, and after many alternations of hope and anxiety, the patient (a child of six and a half years old) entirely recovered, the false membrane having been discharged through the wound, partly by the efforts of coughing and partly by extraction by the forceps. In another case, however, where the operation was also performed by Dr. Jules Simon, and apparently with perfect success, the patient died suddenly and unexpectedly from syncope, a fatal result which follows diphtheria almost as frequently as death from suffocation does, and such a case must always serve as a warning to medical practitioners to exercise the utmost caution in giving a prognosis in this disease, however favorably the patient may seem to be advancing.

In another chapter Sir John Cormack thoroughly discusses the relation existing between "diphtheria" and "croup," and he arrives at the conclusion that the former is characterised by the presence of a false membrane, while the latter is only a name given to a symptom common to many affections. He enters largely into the literature of the subject, both foreign and British, in support of his views, which he illustrates, moreover, as we have noticed above, by the lessons of his own extensive experience.

The "Paralytic Affections of Diphtheria, their Nature, Course, Pathology, and Treatment," form the subject of another chapter, and are illustrated by some typical cases which have fallen under the author's observation. In connection with the pathological bearings and analogies of these cases, it is shown in another chapter that paralytic affections may and do occur during and after enteric fever, and further on in the book the subject is continued by showing that these same affections may also accompany or follow relapsing fever, cholera, dysentery, and smallpox. Thus it appears to be proved that there is nothing peculiar in the nature of diphtheria to account for the paralysis so often found as one of its sequelæ, but that "the pathogenesis," as Sir John Cormack writes, "of most—I do not say of all—of the paralytic affections following acute diseases is no doubt the same." If so, their treatment must necessarily be the same, and as the dominant condition in all these cases is asthenia, the rules of treatment are pretty obvious. It would appear, however, that asthenia and anæmia are peculiarly frequent during the convalescence from diphtheria, and a French author has arrived at the conclusion that in every eleven cases of this disease one is characterised by paralysis.

The rest of this second volume is made up of chapters on a great variety of subjects, namely, "Non-Venereal Discharges from the Genito-Urinary Organs of Both Sexes;" "Scarlatinal Vaginitis with Muco-Purulent Discharge;" "A Case of Congenital Syphilis;" "A Case of Recovery from Poisoning by Chloroform." A peculiar interest

is attached to the case of "Successful Resection of the Shoulder-Joint in a Case of Gun-shot Wound," from the fact that the incident related occurred during the dreadful days of the Communistic Insurrection in Paris, when Sir John Cormack was honorably and usefully employed in rendering medical and surgical aid, in connection with the Ambulance Anglaise, to the combatants in that memorable struggle. The remaining subjects treated are, "a Case of Concussion of the Brain;" a chapter on "General Paralysis of Insanity;" and the final chapter treats of "Short Attacks of Insanity in Women."

X.—Biological Contributions to the Royal Society.

As the Royal Society includes amongst its Fellows men who are actively studying every branch of natural knowledge, its publications must contain a wonderful variety of subjects. According to the precept of the society, the term natural science includes all kinds of investigation except those of pure literature, religion, and metaphysics. It embraces mathematics in the most general sense, physics, chemistry, geology, and the natural history sciences; and therefore, the reader of the 'Transactions' never fails to have both variety and novelty. Take as an example the first of the last three volumes, which have appeared with a slowness hardly consistent with the example set by other societies, and on its contents-page will be found the following:

"On a standard voltaic battery; on the dynamics of a rigid body, &c.; on the organization of the fossil plants of the coal-measures; on the action of electricity on gases; on the anatomy and histology of the land Planarians of Ceylon; on a newly discovered extinct ungulate mammal from Patagonia; on the atmosphere as a vehicle of sound; on the fossil mammals of Australia; and on the structure and development of the skull in the pig."

Certainly this list disproves the generally received idea that the Royal Society is for the abstract mathematician and physicist alone, for there are five biological subjects out of nine. In the next volume there are ten papers which are of interest to the biologist out of twenty-four, and in the last which has appeared there are five natural history contributions out of ten. Moreover, in the 'Proceedings' which come out several times in the year, there are at least 16 per cent. of papers which are biological, and this calculation omits all the communications relating to the deep sea. There is no doubt that this very satisfactory development of biological science is due to the present attainments of the rising generation of medical men, and especially to the results of the scientific impulses

of the University of London. The number of M.D.s who present themselves for election to the Royal Society is ever on the increase; and it is gratifying to notice that most of them do not anticipate a complimentary election, in consequence of their having physicked the aristocracy, or of having introduced princes and princesses into this world of troubles, but expect to compete with the world of science in the field of original research. Some years since, the president of the day, Sir E. Sabine, remarked, on having the list of candidates read out to him, "What a lot of doctors," Sharpey answered very properly, "The principal introduction to science nowadays is through the medical profession." But there are some very rising naturalists and physiologists who, thanks to the Universities of Oxford and Cambridge, are studying biology, not with the view to practise the curative art, but to take the novel line of being biologists pure and simple. The new physiological laboratories and the incentives to research are stimulating students to investigation, and the work is progressing admirably. There is but one complaint to make, and its cause is evident enough in reading the contributions of the younger biologists; it relates to the incomprehensible jargon in which they often clothe their descriptions of facts—a scientific slang, highly scientific of course, but foreign. A thing to be altered at once; for it is well known that he who describes in the plainest Saxon is the most able and comprehensible. Many of the biological contributions of these two classes of observers are not by fellows of the society, but they have been considered sufficiently meritorious to be published. Some are even found in the 'Philosophical Transactions,' so that the world outside the Royal Society cannot complain of its exclusiveness as regards admitting the work of conscientious workers in everything relating to natural science. One thing is certain, and that is, when a memoir is sent to the Royal Society it will be read and criticised; but another matter is by no means as certain, and that is, when the essay will be published. And after all this there is a very small chance of any medical periodical taking the slightest interest or notice of the subject. Under these circumstances a review of some of the more important papers which have appeared of late years may present the aspect of novelty.

Studies on Biogenesis. By William Roberts, M.D.—This paper was communicated by Prof. Roscoe, F.R.S., and it is an admirable instance of careful experiment and clear writing. Its influence is still felt, and its broad spirit of inquiry leaves some points relating to "spontaneous generation" still open. The author notices that the question of the origin of Bacteria and *Torula* lies so deeply at the root of some of the most important problems, not only of biology but of pathology and practical therapeutics, that he made no apology for bringing forward his researches. He stated the question thus:—"The main question in controversy is whether these organisms origi-

nate *de novo* in the media where they grow, or whether they spring like higher beings from germs or parents like themselves." Spores or germs, or no spores and germs. One school insists on their presence everywhere in the air and water, another decides that they are not necessarily present, and a third inculcates that organic matter can assume the shape and potentialities of the lowest forms of life without germs or generation *ab ovo*. Dr. Roberts wisely acknowledges that panspermism, *i.e.* the universality of sperms, is not absolutely antagonistic to the theory of abiogenesis or the origin of life without the sperm, and suggests, as many have done, that "It is conceivable that while the ordinary and common origin of Bacteria and Torula is by procession from pre-existing germs, there may also be conditions in which they arise *de novo*." He then proceeds to establish the fundamental propositions of the germ theory, by dividing his work into three parts. In the first the conditions under which organic liquids and mixtures are rendered barren by heat are detailed; in the second the question is investigated whether the normal juices and tissues of animals and plants are capable of producing organisms without infection by extraneous germs; and in the third part the facts adduced in the two previous sections are considered in their bearing on the origin of Bacteria and Torula, and some of the alleged cases of abiogenesis are tested experimentally.

In sterilizing organic liquids and mixtures by heat Dr. Roberts found that on boiling beef tea or decoction of turnips for a few minutes in a flask, the neck of which is plugged with cotton wool, the liquids pass into a state of permanent sterility so long as the cotton-wool plug remains undisturbed. The sterile liquid has not lost its power of nourishing living things, for if the wool be removed for a short time Bacteria, Vibrio, &c., soon appear in it and grow and multiply with their ordinary luxuriance. The condition of permanent sterility described by Dr. Roberts is essentially characterised by loss of the power of *originating* organisms, but it is by no means antagonistic to their growth from germs coming from without. Different organic liquids and mixtures require different amounts of heat to produce sterility; milk, according to Pasteur, requires more heat than sweetened yeast water, and Bastian has shown that turnip infusion and cheese cannot be sterilized, as an ordinary decoction can, by boiling for five or ten minutes. Dr. Roberts shows that there are liquids which germinate after the exposure to the heat of boiling water for even two and three hours, or after the exposure to a heat of 9° Centigrade above that of boiling water for more than half an hour. It appears that there are two factors of equal importance in the process of sterilization, namely, the degree of heat and the duration of its application, and the factors are mutually compensatory. Slightly alkaline liquids were shown to be more difficult to sterilize than slightly acid ones, and this bears some relation

to the ready coagulation of albumen in acid solutions. But Dr. Roberts states—

“Sterilization generally proved to be permanent if germination did not occur within four days. Sometimes, however, germination took place on the sixth or eighth day, and very rarely even as late as the tenth and twelfth day. Differences in the degree of warmth at which the preparations were maintained (after the heating) accounted for most of these cases of retarded germination, but not for all. Moreover, manipulatory difficulties determined some error, but still the retarded germination in some cases could not be referred to those causes. As a rule, if germination took place at all it occurred as promptly in solutions which had been boiled for long as for short periods.”

Some very careful experiments were made on this part of the subject and are recorded at the close of the essay, and they show that some organic liquids kept in flasks after boiling, the flask being plugged with cotton wool, after being sterile for months, have no Bacteria developed in them, but *Penicillium* and the *Torula* series. In some ninety experiments which Dr. Roberts succeeded in performing, his results indicated that the normal tissues and juices have no inherent power to originate organisms, and that when these appear in them they have come from without. He showed also that there are particles in the air and in water which in certain liquids are capable of preserving their germinal activity after an exposure to a boiling heat for five minutes, and some forms of Bacteria or their germs are capable of surviving this heat.

In considering the bearings of this communication on the theory of spontaneous generation, which has been so constantly before the scientific world, it is evident that the impossibility of Dr. Bastian's views being true is not admissible. There are, however, some elements of error in the experiments, which will be noticed further on in considering a paper by Professor Tyndall, and which relates to the possible impurity of the cotton-wool plugs. On the other hand, Dr. Roberts quotes the interesting statement of the exact observers, Drysdale and Dallinger, regarding the extreme minuteness of the spores of *Cerco-monads*. It is somewhat as follows:—The spores of this organism are so minute that they cannot be individually recognised, even with a 1-50 inch objective. They can only be recognised with this high power as seen in enormous aggregation and motion in a mass. With a magnifying power of 2500 diameters they are only just visible as the minutest dots. These observers showed that there was a vast difference between the vitality under heat of the *Cerco-monad* and its spores, for whilst adults were killed at a temperature of 60° Centigrade, their spores survived a temperature of at least 127° Centigrade.

Dr. Roberts concludes his essay with the expression of the opinion that the facts just enumerated are far too few and of too exceptional a

character to permit a deduction in favour of abiogenesis ; but they certainly impose a reserve which is highly significant. If future investigations should establish the occurrence of abiogenesis (spontaneous generation), this would not overthrow the panspermic theory ; it would only limit its universality, and it may be predicted with some confidence that if abiogenesis exist, the conditions of its occurrence can only be determined by an inquirer who is fully alive to the truth and penetrating consequences of the panspermic theory.

A communication in the 'Proceedings' by John Tyndall, F.R.S., carries on the argument about panspermism, and the experiments detailed in it and the results obtained are summarised in the professor's happy manner. Feeling that the medical, clerical, and general intellect had become obfuscated by the complexity of the subject of spontaneous generation, "he wished to free his own mind, and, if possible, the minds of others, from the uncertainty and confusion which now beset the doctrine." After noticing the doctrine of Pasteur, that spontaneous generation was a chimera, and after quoting Messrs. Braidwood and Vacher's opinion regarding the generation and communication of contagion, as stated in the 'British Medical Journal,' the author proceeds to diminish the dull uncertainty in his own mind by some elaborate experiments with the electric beam of light, and close boxes smeared with glycerine at the bottom.

He showed that at first the air in the box was full of suspended dust (by means of the ray of light) but that after three days it had all sunk to the bottom of the box, and had stuck to the glycerine. Then test-tubes were filled with organic liquids and boiled in the box, filled as it was then with "moteless air." Under the circumstances there was the usual air of London, with oxygen, nitrogen, carbonic acid, ammonia, aqueous vapour, and all the other gaseous matters peculiar to the air of great cities. Moreover, the air had not been tortured by calcination or filtration ; and the question to be solved was, Can the air produce putrefaction? First of all, the same kinds of boiled organic liquids in test-tubes were exposed in the laboratory of the Royal Institution without confinement in a box, &c., and they one and all began to putrefy in from two to four days. Then the result was noticed with regard to the prepared tubes, and in no single instance did the air which had been proved moteless by the searching beam, prove itself, even when raised to temperatures of 80° to 90° Fahrenheit, to possess the least power of producing Bacterial life or the associated phenomenon of putrefaction. The power of developing such life in atmospheric air and the power of scattering of light are thus proved to be indissolubly united. The sole condition necessary to cause these long-dormant infusions to swarm with active life is the access of the floating matter of the air. After having remained for four months as pellucid as distilled water, the opening of the back door of the protecting case and the

consequent introduction of the mote-laden air sufficed in three days to render the infusions putrid and full of life. That such life arises from mechanically suspended particles is thus reduced to ocular demonstration. Influenced by the value of the test of the electric beam, Professor Tyndall notes that only the finest of the visible motes may be the developers of Bacteria, and insists that they may be much too small to be seen by the microscope. He adduces the coloration of water by alcohol and mastic or eau de Cologne, and the impossibility of separating the colour-giving particles by the microscope as a proof that there are limits of the distinctness of solids under the highest powers. This is rather too much, for it goes towards the theory of the invisible nature of sperms and to all sorts of paradoxes, and the fallacy of the argument is obvious. Professor Tyndall's hypothesis is that the coloration is produced by the colour of light on solid matters, but it is by no means clear that there may not be a condition of fluids which is productive of colour, but where there is no solid. He then decidedly contradicts the opinion of Dr. Bastian regarding the production of Bacteria and Vibriones in organic infusions which have been boiled, and over which the air has been filtered and coloured, &c. He states that the doctor is human, and that it is human to err, and that his experiments were not conducted with critical care. He then suggests that Dr. Roberts's cotton wool was impure, and that any equivocal results are due to the few germs in some cotton-wool plugs. After noticing the importance of the work to medical men, especially in typhoid fevers in cesspool neighbourhoods, Professor Tyndall insists on the universal prevalence of the germinal matter of Bacteria in water, and states that Dr. Burdon-Sanderson has demonstrated it "with the utmost evidence," whatever that may be. He goes on to state— "But the germs in water are in a very different condition, as regards readiness for development, from those of air. In water they are thoroughly wetted, and ready under proper conditions to pass rapidly into the finished organism. In air they are more or less desiccated, and require a period of preparation more or less long to bring them up to the starting-point of the water-germs."

Finally, the author of this interesting paper remarks that the particles in air differ widely amongst themselves as regards preparedness for development. Some are fresh, others old; some are dry, others moist. This will explain the different periods of putrescence in liquids.

In reading this essay one is struck with the refinement of the first experiments and the use of the electric beam, and of the conclusive proofs that in boiled organic liquids putrefaction cannot occur without the tainting with germs or organic stuff turning into Bacteria from without. But there is much, too much, of the subsequent work and statements, which is mere conclusion jumping; much is

irrelevant, and all is inconclusive. For, after all, the experiments only prove what many other observers have stated, and they really do not advance on Dr. Roberts except in the explanation referring to the cotton wool, which must be taken as not too conclusive, as the cotton wool in all the other instances was good and did its filtering work. What has been proved by these essays is—(1) in a sterile liquid surmounted with air freed from germs cannot putresce or develop life, and certainly no one ever thought it would; (2) boiling renders organic liquids sterile, and any subsequent Bacteria, &c., must have been introduced or have been air-carried. It is evident that unless nearly all the opportunity and possibility of abiogenesis are destroyed by the application of heat, these experiments are of no value, and therefore they are not conclusive. Granted that germs are omnipresent, it still remains probable that an organic liquid may develop Bacteria without them. With regard to the omnipresence of germs, how is it that aquaria do not teem with Bacteria and Vibrios? and how is it that during investigations under glass covers into the nature of many low forms of life the organic liquid often becomes dulled, and then suddenly teems with moving monads?

The potentiality for assuming a definite form and receiving certain vital energies on the part of organic fluids, analogous in a degree to crystallization of inorganic liquids, has not been proved, but it probably has not been disproved by Pasteur, Roberts or Tyndall, and therefore we pass on to some other essays bearing on the subject.

In 1876 Dr. Bastian, F.L.S., contributed "Remarks illustrative of the Physico-chemical Theory of Fermentation, and of the Condition forming Archebiosis in previously Boiled Fluids." This author notices that previous investigators had observed that neutral or slightly alkaline organic fluids are rather more prone to undergo fermentation than slightly acid fluids. After having satisfied himself that this was correct, he commenced some experiments to ascertain whether the fermentability of boiled urine, like that of many other fluids, could be increased by previously mixing with a quantity of liquor potassæ sufficient for its neutralization. The experiments answered this question in the affirmative; and then further experiments were instituted to throw light upon the cause of the increased fermentability. It was desirable to ascertain whether it was due to the revival of germs in the boiled neutralised fluid or to the chemical influence of the potash in initiating or helping to initiate the molecular changes leading to fermentation in a fluid devoid of germs or other living matter. The mode of testing the relative validity of these novel interpretations seemed easy, and Dr. Bastian writes—

"It was only necessary to ascertain what the effect would be of

adding boiled liquor potassæ in proper quantity after the acid urine had been rendered barren by boiling it, instead of adding it previous to the process of ebullition. If fermentation occurred in the fluid thus neutralised without extraneous contamination, the first interpretation would obviously be negatived."

This crucial experiment was tried, and on several occasions it was noticed that the fermentability of urine was just as much increased by adding boiled liquor potassæ after the urine had been boiled in the acid state as by adding the alkaline beforehand. Such a result is opposed to the first interpretation of the cause of the increased fermentability of acid urine, and of course enhances the value of that which relates to spontaneity. By adding slightly more liquor potassæ than is necessary to neutralise, the liquid remained barren; but by adding a larger quantity, the development of Bacteria may take place, so that it seems that this agent mixed with boiled urine in quantity slightly more than is needed for neutralisation prevents the *origination* of living matter therein, although even when in considerable excess the same agent affords no obstacle to the development, growth, and multiplication of germs purposely added thereto. These experiments, writes Dr. Bastian, "show that an exclusive germ theory of fermentation is untenable, and that living matter may and does originate independently during the progress of fermentation in previously germless fluids."

This paper was a kind of challenge to Dr. Roberts, who in reply communicated his results to the 'Proceedings,' and they "negative the conclusion that liquor potassæ or a temperature of 115° to 122° F., or both conditions combined, have the power of exciting the generation of organisms in sterilized urine." He confirms Dr. Bastian's views with regard to the growth and multiplication of Bacteria in unsterilized urine when exposed to a temperature of 115° to 122° F.

Finally, with regard to this question, Tyndall experimented on the plan adopted by Dr. Bastian, and he records Pasteur's experiments on cognate subjects, all being against Dr. Bastian's views. At present, then, the verdict is rather "not proven" than anything else; but the influence or non-influence of elevated temperature on germs is a great addition to science.

There is an essay in the 'Philosophical Transactions,' dated 1874, and published in 1875, by Dr. E. Klein, assistant professor at the laboratory of the Brown Institution, London. It was communicated by Mr. John Simon, who was at that time Medical Officer of the Privy Council, inasmuch as Dr. Klein was not a Fellow. It is interesting to the medical profession and to histologists of the advanced school, as it considers the structure of the pustules of smallpox in the sheep, with the aid of all the developments of modern research. There is a fine physiological laboratory, funds, abundance of time,

microscopes, all sorts of preparing fluids, and the necessary sheep and lymph, and the result has been a fungoid theory of the small-pox, the recognition of a new fungus called *Oidium variolæ*, Klein, and—a distinguished failure. The essay is unsparing in the recognition of forms of life which the strict investigator of fungi deems superfluous, and the unmeaning terms *Micrococci*, *Cryptococci*, *Microsphæra*—terms which relate to no definite organism, but are the cells of any number of fungi, or are not fungi at all—are used in a manner sufficient to drive a botanist to distraction. Of course no notice is taken of any British observer of these minute forms, and we cannot help thinking that if they had been placed in the hands of some of our authorities on the subject Dr. Klein would have been spared the unsatisfactory result of mistaking a mycelium for a physical condition induced by the employment of chromic acid. There is much good work in the essay, and much clear demonstration. It commences with a description of experiments with fresh lymph (variolous) by inoculation of sheep. On the fifth day an areola surrounded the punctures, and on the morning of the next day a new pock appeared, and one in the evening. They increased rapidly in size, the red areolæ becoming larger in breadth and in thickness. After the first two days of their appearance they only became more elevated, *i.e.* thicker. As long, writes Dr. Klein—

“as they increased in breadth they nearly all showed the central part most elevated, but as soon as they ceased to increase in breadth, or shortly before that, they became depressed, and at the same time pale in the centre, whereas the peripheral part seemed now to be very much elevated and red.”

This is, of course, what was to be expected, and Dr. Klein in investigating the subject carefully describes the cause, and with his usual ability, in another part of the essay. The second part relates to the anatomical method, and this must be stated, as it was quite in the received groove of the present preparation mania, and as it led to the failure. The pocks were cut out with a small zone of healthy skin and pinned out on a cork like a tent, the pins being fixed in the surrounding healthy skin, and the object was then placed cork upwards either in $\frac{1}{4}$ or $\frac{1}{3}$ per cent. of chromic-acid solution or in methylated spirit. Four or five days were usually quite long enough to bring the object to such a consistence that it could easily, when embedded in a mixture of wax and oil, be cut into microscopical sections. Before cutting the pocks, which had been hardened in chromic acid, they were placed in spirit for several hours. Now, it is quite evident that no satisfactory result regarding the fungoid nature of the pock could be the outcome of this exposed method. At this point the paper diverges, and there is an investigation of

the organisms contained in fresh lymph by cultivation. The previous investigations of Cohn, Heber, Hallier and Zürn, Chauveau, and Sanderson, are noticed, and had Dr. Klein been acquainted with the polymorphic unicellular fungi and with the utter want of value as definite species of the entities noticed and described, he would have spared himself their repetition. He repeats Hallier's notion :

"As the sporids originating from micrococcus of sheep-pox develop in the air to *Cladosporium*, which is identical with one of the forms of *Pleospora* recognised by Tulasne as the conidium-bearing forms, and as this latter, a parasitical fungus living on *Lolium perenne*, is contained in spoiled hay, the inference which may be drawn from this is obvious—that spoiled hay is the source of the infection of sheep-pox."

But he justly recognises the merit of Chauveau, who deduced the presence of organic particles being the carriers of the contagion in the lymph of vaccine as well as in that of sheep-pox, and also Burdon-Sanderson's confirmation of this. According to Sanderson, these organic bodies are certain spheroids which are properly called mycrozymes, a term acknowledged by microscopic botanists—while micrococcus is not. These microzymes are capable of movement, and their relation to Bacteria has been acknowledged. But we decline to follow their supposed proliferation, as the manner of manipulation is suggestive of all sorts of errors. The pale spheroids are facts, but their increase by subdivision and the formation of colonies or Zooglæa is open to great doubt. Cohn calls the supposed derivatives *Microsphaera vaccinae*, and classifies them in a family, of which we are ignorant as British botanists, in a group as Bacteriaceæ, just as if Bacteria was not an expression of a score of vegetable entities. Dr. Klein then gives his own results. Clear lymph, which had been kept for several days in a sealed capillary tube, was diluted with thoroughly boiled half per cent. saline solution, and was used thus :—One portion of the diluted liquor having been reserved for further experiments, the remainder, which was intended for microscopical examination, was sealed immediately after it was prepared with dammar varnish and examined. He got in this preparation Bacteria-like bodies; minute, highly refractive spheres isolated, or in couples or small groups, which corresponded to the *solid* granules (micrococci) of Cohn. They presented the *Brownian* movement. There were also decolorised blood-corpuscles. After the preparation had been kept for twenty-four hours at a temperature about 38° Cent., the number of spherical bodies increased, and they formed into groups and assumed a fibrillar arrangement. Under a high power the fibres are noticed to consist of spherical bodies and granules of different sizes arranged in rows, and each row is embedded in or rather connected by a pale transparent substance. The granules grow into the spherical bodies, the Bacteria disappear, and multiplication

by division proceed. Then Dr. Klein gives the results of a drop of lymph which was obtained from a pustule of an animal which had been infected fourteen days before, and it was examined without the addition of the saline solution. It showed large numbers of granular pus-corpuscles, coloured blood-corpuscles, numerous small highly refractive granules isolated and in couples, and exhibiting molecular movement. Then it was placed in the incubator for twenty-seven hours; temperature 32° Cent. The results showed—

“ Besides intact granular pus-corpuscles there were numerous pus-corpuscles, the substance of which had become swollen and transparent; these contained two to six spherical, homogeneous, not very highly refractive bodies, about half the size of a coloured blood-corpuscle or even less. Some pus-corpuscles containing these bodies were seen to be in a state of becoming disintegrated, and through them spherical bodies becoming freed. That they are not nuclei of pus-corpuscles is shown by the fact that they become the more distinct the more the matrix of the pus-corpuscle becomes swollen and disintegrated. They are most distinct when they have become freed from a corpuscle. Besides, they have a *slightly greenish colour* and are homogeneous, whereas it is well known that when pus-corpuscles swell, also their nuclei become swollen and have the appearance of vesicles surrounded by thin membrane. Similar spherical bodies are found in the surrounding medium.”

From these forms one can trace others which possess one or two small dark granules; in the latter instance the corpuscle is somewhat elongated, and the granules are situated at its pole. These spheres become transparent and granules make their appearance on them; these are highly refractive minute spheroids and are micrococci!!! Dr. Klein says they multiply by transverse division, and by some means, which are not clearly defined by him in his paper, they get out of the spheres and form chains, which are held together by a transparent connecting substance. Then they become convoluted and formed into a colony or zooglæa. Under heat the so-called micrococci enlarge and apparently are developed into spheres, or else spheres come out of the organic matter. He considers the spheres to be identical with the microzymes of Sanderson, and these to be micrococci in a dropsical condition. Finally, he decides that the filaments or chains have a genetical relation with micrococci. With great respect for Dr. Klein, it is still hard to believe that a vegetable sphere can arise in a pus-cell, the fungoid granules (micrococci) within it, and that these, as fungi, shall form chains and colonies. The large spheres in pus-cells are common enough in some strumous fluids, and they are the parents of highly refractile granules, which, under common circumstances, turn to pus-cells by aggregation or to great granular aggregations.

It is this straining to develop a fungus which produced bad fruit in due time.

The next part of the paper contains an anatomical investigation of the eruption, and the labours of previous observers are considered. We plunge at once into the fungoid theory. Luginbühl states with delightful dogmatism his views, which are faithfully recorded and diametrically opposed by Dr. Klein. The first-mentioned observer makes the whole process start in the epidermis, and hence gradually to extend to the corium, a statement, according to Dr. Klein, wholly inconsistent with the facts of the variola of sheep, the phenomena of which are analogous to those of men. Then, the results of Auspitz and Basch, as confirmed by Neumann, are given, and in a footnote it is stated that they found micrococci in the fully developed pustule and in the corium, another observer having detected them in the lymphatics.

The principal alterations during the development of a pustule are, according to Dr. Klein, increased thickness of the rete Malpighii, whose cells become more transparent and larger, and have two nuclei, so that the transparency is very evident when the skin of the pock is in contact with healthy skin. The papillæ become more distinct in the corium, whose blood-vessels are distended, their endothelium being distinct to view. The tissue of the corium is œdematous and the lymph-canals are visible, distended and filled with a transparent homogeneous or purely granular substance, which resembles coagulated plasma. The changes of the subcutaneous tissue are similar but slighter, and they diminish with depth. In twenty-four hours all these appearances become much more marked, the nuclei have divided, the œdema has increased, and the plasma has coagulated, and lymph-corpuscles, probably decolorised blood-discs, infiltrate the corium and are traceable to the larger branches of the vessels. The connective-tissue-corpuscles have enlarged and the nuclei are divided or are in a state of division. Finally, the lymph-canals contain not only a few lymph-corpuscles but also a finely granular material, in which are embedded *peculiar ovoid or spherical, sharply outlined structures, containing in a clear substance one large, or two, three, or four small highly refractive bodies*. These structures are of different sizes, the smallest being not smaller than the nucleus of a connective-tissue-corpuscle, the larger being twice or thrice the size. What are these sharply outlined structures? Dr. Klein, inoculated with the germ theory, declares them to be the cryptococci of Hallier. In proof of this he proceeds to notice that in some lymphatics in the deeper part of the corium, which are filled with a transparent matrix, there are highly refractive granules *arranged in shorter or longer, branched or unbranched, filaments*. At a later stage the filaments, conidia, &c., came out beautifully and were undoubtedly parts of a fungus. They were the mycelium and the reproductive elements,

and the new form comes before the world as *Oidium variolæ*. Perhaps there might be a connection between bad hay, sheep, and the vine disease! Who can tell the depths of the mysteries of the polymorphic protista?

The rest of the paper is taken up with an admirable description of the changes in the layers of the skin in the production of the area of depression and the surrounding limit of swelling; the drawings are exquisite, and all except the "fungus" part is well worthy of a very painstaking and distinguished histologist.

How the paper as a whole got into the 'Philosophical Transactions' is to be wondered at. It was communicated by Mr. John Simon, and therefore that gentleman could not have been its referee. Who was the botanist to whom the matter was referred? Well, two years after the paper was read, Dr. Charles Creighton communicated a notice to the society "On certain unusual Coagulative Appearances found in Mucous and other Albumenoid Fluids." He showed that under the influence of *certain reagents* the albumen becomes granular or appears as droplets or granules in mucus, and that the clusters of granules "further assume more of a reticular arrangement or more of a linear, according to circumstances." "The plasma of the blood is found sometimes in chromic acid preparations to assume the same coagulation forms as those just described and explained in mucus; the reticular-like or nodulated fibres are obviously a modification of the ordinary fibrinous filaments of blood-clot." The author then distinctly affirms that some of Dr. Klein's figures cannot be distinguished in any point from the particular coagulative appearances that he found in the ducts of the mammary gland!! He tells Dr. Klein roundly that his "circumstantial account rests on erroneous observation and on defective evidence." To end this very instructive episode Dr. Klein received intimation of Dr. Creighton's correction and sent in a paper which is published in the 'Proceedings' of the Royal Society, and he acknowledges that there is no mycelium, but a physical change brought about by chromic acid, and that the vegetable nature of the other substances becomes doubtful. Thus, *Oidium variolæ* returns to its parent form "error," and affords another instance of the folly of the investigators who forget that in making tissues more easy of manipulation they destroy, alter, and manufacture pathological elements. Had Dr. Klein carefully examined the pocks without chromic acid he would have recognised all the modifications of cell proliferation under a special hyperæmia, and he would have escaped plunging into errors which excite the risibility of any adept in the mysterious regions of the lower Plantæ.

Dr. Cresswell Baber communicated a paper "On the Minute Anatomy of the Thyroid Gland," and the abstract of it in the 'Proceedings' shows that it is of much interest to histologists. He directed

his attention mainly to the lymphatics, which were injected (in the dog) with Berlin blue by the method of puncture. He states, "Traversing the gland chiefly in a longitudinal direction are large lymphatic vessels provided with valves. In direct connection with these, and permeating the gland in all directions, is a dense meshwork of lymphatic tubes and spaces. The smaller lymphatic tubes run between individual gland-vesicles, the larger between groups of the same." They expand occasionally into irregularly shaped lymphatic spaces, but present no blind extremities, as stated by some authors. Injections with nitrate of silver showed an endothelium to the lymphatic system. The tissues between the alveolæ were studied, and there resulted the discovery of a *parenchyma* of large rounded cells, each provided with an oval nucleus and found either singly or in groups amongst the epithelial cells. This appears odd enough, but the author considers that these great cells make their way through pressure from without into the vesicles, and of course then come in contact with the epithelium. He comes to the same conclusion which we were taught more than a quarter of a century since, that the physiology of the thyroid and that of the blood are in direct relation. This paper is followed by Mr. Herbert Watney's, "On the Minute Anatomy of the Alimentary Canal." This is abstracted in the 'Proceedings,' and the first part considers the minute anatomy of the mucous membrane of the small intestine and the method of fat absorption. After giving a notice of the researches of previous observers, he states his method of hardening and staining, and proceeds to describe the minute structures of the mucosa. He notices that lymph-corpuscles are constantly found among the epithelial cells of the intestine, and that the epithelium contains a *reticulum* continuous with that of the mucosa. This reticulum appears in horizontal sections of the epithelium as small branched cells, which sometimes show a nucleus. In vertical sections the reticulum is seen as a dark line at the base of the epithelium, and from this fine processes run among the epithelial cells. The reticulum is stained very deeply by chloride of gold and by hæmatoxylin. If teased, shreds of it are seen attached to separate cells of epithelium. These are closed at their free extremities, and the author states that young epithelial cells are due to "germination" or sprouting, and, we suppose, from already formed cells. In considering the villi, it is found that the *membrana propria* is composed of large cells, which in vertical sections are round or oval, and lie close below the dark line of the reticulum. This breaks off with the epithelium. The most comprehensible statement is made with respect to the Lieberkühn crypts. When isolated, their *membrana* is a homogeneous membrane, with nuclei embedded in it. But in oblique sections of the *membrana in situ* it is seen to be composed of cells, and the reticulum surrounds the

cells composing it. This inter-epithelial structure grows in importance as the author proceeds, for that of the mucosa becomes continuous with that between the muscular fibres; and it is not merely a network of threads, but it also forms membranous investments. Thus, the blood-vessels of the villi have an "adventitia" (why can it not be called a "loose coat"?) continuous with the reticulum and the chyle-vessels also; and finally, to prove that he is dealing with the old-fashioned areolar tissue of a few years back, it is necessary to mention that the author states that "the tissue of the mucosa is shown to be everywhere composed of a reticulum and cells held in its meshes." Mr. Watney considers that fat does not make its way into the epithelial cells, but travels by way of the reticulum. In considering the stomach, there are statements made regarding the closure of the epithelium during inanition, and that a cell is open at its free end during secretion. He believes that the epithelial cells divide; that the small rounded cells (other than lymph-corpuscles) are the products of their division; that these small cells, increasing in size, rise up amongst the older cells, push them to one side, and become short broad cells, and that these divide longitudinally, forming "buds." The reticulum is found in the stomach and amongst the epithelium of the proper gland-tubes. Finally, after noticing alterations in the shape of the nuclei during digestion, the author describes the mucous membrane at the point of transition between the stomach and intestine. Most human and comparative histologists will demur to much of this, and it is certainly inexplicable how the author's descriptions of the epithelium of the intestine and stomach can be made to tally with such excellent work as was done by Brinton with first-rate glasses and a very cool understanding. This essay on the stomach and intestine in the 'Cyclopædia of Anatomy and Physiology' has but one defect. The author of it worked at stomach *au naturel*, and there can be no doubt about the correctness of his descriptions. Mr. Watney has a fine field before him, and especially as he has the opportunity of original research, and not necessarily in the "groove" of the school which is at present in fashion.

The last essay which our space will permit us to notice is one by Mr. G. F. Dowdeswell, "On the Connective Tissue of the Tongue in Inflammation." It is a clever short paper, well illustrated, and written clearly. The author sums up as follows:—"The result may be stated in a single line. So long as the circulation continued, no change whatever took place in the connective-tissue-corpuscles, either as regards form or appearance. Notwithstanding that, this tissue was beset with innumerable emigrant corpuscles, *i. e.* (to use ordinary language) was infiltrated with pus."

XI.—The Health of the Government Prisons.¹

IF the new "Prisons Bill" is to hand over to Government the working of all the gaols of England, the returns of those prisons that have been so long under their management come to possess a special public interest. The following digest of the Medical Reports of the Government Prisons of England for the year 1875 is not supposed in any way to deal with even the medical aspect of what is termed the "convict question." The object here proposed is so to summarise those returns as to reduce them in some measure to an appearance of unity. So far as we are aware, no summary of this sort, official or otherwise, has ever been made, and this is the more surprising as it is impossible to get any clear conception of the health and condition of convicts as a body without studying the statistics of *all* the prisons, taken collectively. In such a service as the convict department is, where the numbers are not excessive, and where discipline keeps the individuals constantly under observation and upon a certain regulated apportionment of food, diet, and daily habits of life, much interesting material for study is furnished, and the medical returns, if properly elaborated, ought to supply not only reliable but valuable information.

The convict prisons of England are thirteen in number, of which two are separate establishments for females, and one (Millbank) contains both males and females. They are all situated in London or along the south of England. The average convict population for 1875 was 9782, comprising 8564 males and 1218 females—a ratio of about 7 of the former to 1 of the latter. From a return (No. 17) given in the appendix to the Directors' report the average length of sentence is given as being eight years in the case both of the male and the female convicts in custody on the 31st of December, 1875.

The following table gives the general details of the mortality and sickness for the year. The total number of *deaths* for the year was 155, being at the rate of 16·8 per 1000 of population. Of this number 135 occurred among the men and 20 among the women, the rate per 1000 being 15·7 among the former and 16·4 among the latter. One female died to 6·7 males, the proportion of the sexes in the whole population being 1 female to 6·9 males. This mortality is somewhat above the average mortality for convict prisons. The mortality reckoned upon the hospital cases was, among the males, 1 death to 43 cases; among the females, 1 to 40. In the whole population there were 6645 cases under treatment or

¹ *Report of the Directors of Convict Prisons for the year 1875.* Presented to both Houses of Parliament. London, 1876.

Table showing Mortality and Hospital Cases in English Convict Prisons for the Year 1875.

CLASS OF DISEASE, &c.	Actual mortality.			Mortality per cent. on total deaths.			Number of cases treated in hospital.			Percentage of hospital cases to population.		
	M.	F.	Total.	M.	F.	M. & F.	M.	F.	Total.	M.	F.	M. & F.
Zymotic Diseases	3	0	3	2.2	0	1.9	375	45	420	6.4	5.5	6.3
Diathetic	63	12	75	46.2	60	48.3	747	147	894	12.7	18.2	13.4
Diseases of Brain and Nervous System	6	4	10	4.4	20	6.4	425	77	502	7.2	9.5	7.5
Circulatory System	14	2	16	10.3	10	10.3	176	13	189	3.0	1.6	2.8
Respiratory	15	—	15	11.1	0	9.6	561	68	629	9.6	8.4	9.4
Digestive	9	1	10	6.6	5	6.4	693	101	794	11.8	12.5	11.9
Urinary and Generative System	8	1	9	5.9	5	5.8	163	33	196	2.7	4.0	2.9
Locomotor Organs	—	—	—	—	—	—	124	8	132	2.1	0.9	1.9
Cutaneous System (including boils, ulcers, &c.)	—	—	—	—	—	—	948	91	1039	16.2	11.3	15.6
Eye, Ear, and Nose	—	—	—	—	—	—	138	25	163	2.3	3.1	2.4
Accident, Injuries, and Operation	—	—	—	—	—	—	637	32	669	10.9	3.9	10.0
Suicide	5	—	5	3.7	0	3.2	—	—	—	—	—	—
Anomalous (including debility, old age, &c.)	12	—	12	8.8	0	7.7	853	165	1018	14.6	20.4	15.3
Total	135	20	155	100	100	100	5840	805	6645	100	100	100

observation in hospital during the year. Of these, 5840 cases were among the males and 805 among the females. The daily average number under treatment was, among the males, 542 or 63 per 1000, and among the females 88 or 72 per 1000 of population.

The standard of illness which necessitates treatment in prison hospitals is by no means the same as that met with in the ordinary hospitals of the country; it is on a lower level in the former, *i. e.* the prisoner has frequently to be taken into hospital for an ailment too trivial for admission into an ordinary hospital. This is more especially the case at public-works prisons, where convicts are employed at more or less heavy work out of doors.

The convict, in these circumstances, if he is not under prison punishment, must be either at work or in hospital as not fit for work. The sickness at public works is greater for other reasons, the principal of which are, firstly, the greater exposure to climatic influences and changes of temperature; secondly, the greater liability to injuries; and thirdly, the stronger motives for malingering and skulking.

The returns do not give the *data* whereby we might ascertain how many individual prisoners out of the whole population received hospital treatment.

The medical report for Portsmouth states that "the admissions to hospital during the year numbered 258, 191 prisoners being admitted once, and 29 more than once." So that the 258 admissions were distributed among 220 prisoners. It would not be very difficult, we think, to get a return for the whole convict department, showing how many prisoners had received hospital treatment, once, twice, or oftener, and how many had not been in hospital at all, and also the number of invalids not actually hospital cases. A series of returns of this sort would not only be interesting in themselves, but they would be useful as a comparative statement of the actual amount of health and disease among convicts from year to year.

We shall now proceed to work out in brief detail the mortality and diseases of the prisons under the various classes of disease given in the above table—so drawn up as best to accord with the nomenclature used in the medical statistics.

I. *Zymotic Diseases*.—There were only three deaths in this class, and it is a noticeable feature in the returns how comparatively exempt convict prisons are from diseases of this nature. Two of the deaths were from erysipelas and one from pyæmia. Of the 420 hospital cases as many as 337 were due to catarrh and febricula.

There were 40 cases of erysipelas, 12 of which were at Chatham and 9 at Parkhurst, the former being a public-works prison for able-bodied men, and the latter being a prison for invalids. No case is recorded of smallpox or of enteric fever. There were two

cases of typhus fever at Parkhurst, one case of scarlet fever at Pentonville, and three at Woking female prison; and of these the "eruptive" category is made up.

The following remarks by the Medical Officer of Millbank are of interest :

"The prison continues to be almost free from diseases of the miasmatic order, and this is equivalent to stating that diarrhœa and dysentery, smallpox and erysipelas, typhus and typhoid fever, are all but extinct as causes of mortality. With regard to the mortality from typhoid or enteric fever, the contrast between the ten years 1845-54 and the twenty-one years following is very remarkable. During the first period there were 57 deaths from that disease; during the second only 3. The improvement in the water supply which was effected in 1854 explains this great difference, and constitutes the separating line between the two periods. Prior to that date the prison had been supplied with Thames water pumped in from the river immediately opposite the gates, and afterwards filtered. This supply was cut off and water from the artesian well in Trafalgar Square took its place, with immediate benefit to the health of the inmates of the prison and a large reduction in the rate of sickness and mortality. Fifteen years have now elapsed since a case of typhoid fever originated here, and it may therefore be fairly said that in this prison that disease is extinct. Millbank, however, by no means stands alone among the Government prisons in this respect. An enviable security is enjoyed, but it is not monopolised. On looking through the annual medical statistics of these prisons for the twenty years 1855-74, I find that only 8 deaths are ascribed to enteric fever, or 40 per annum to a population, male and female, averaging 7913. During the five years 1870-4 only two deaths were caused by this disease in an average population of 9509. If the sanitary securities enjoyed by the inmates of the convict prisons could be extended to the general population of the country, many thousands of lives would every year be saved."

II. *Diathetic Diseases.*—The deaths coming under this head were 75 in number, 63 males and 12 females. With five exceptions (three men and two women) all these deaths were due to one or other of the forms of phthisical disease, to which we shall therefore limit our remarks. Phthisis, beyond doubt, is the most prevalent of all grave maladies among convicts. During the year under consideration it was the immediate cause of 45 per cent. of the deaths. Of the 60 men that died of phthisis, 8 died during the first year of their imprisonment in Government prisons, 8 during the second, 12 during the third, 12 during the fourth, 11 during the fifth, 5 during the sixth, and 4 during the seventh year. Thus, all the deaths from this disease in males happened before any of them had finished seven years, and five sixths happened under five years.

Of the 10 women 8 died under five years, the other 2 in their seventh and eighth years respectively.

The following were the ages at death :

	M.	F.
Under 20 years	5	1
21 to 30	28	3
31 to 40	13	3
41 to 50	9	2
Over 50	5	1
Total	60	10

The state of health on reception into Government prisons of those who died from consumption was as follows :

	M.	F.
Good	41	7
Fair	4	2
Indifferent	5	0
Delicate or bad	10	1
	60	10

We confess we would not have been surprised to see a larger proportion of moderate or ill health on reception. That fully two thirds of the men and of the women should have started in "good" health is more than we would have expected. The corrective tendency of regular living and wholesome food—where the work and diet are fairly proportioned—ought rather to act beneficially than otherwise, especially where we have such favorable sanitary conditions as those we have just noticed.

Unfortunately the per-centage of deaths ascribed to phthisis among convicts does not represent the full extent of its destructive operation, for there is not the least doubt that it contributed to a fatal issue in many cases, as a complementary agent at least, by reducing the physical capability of resisting the influence of other diseases.

The number of cases of phthisis treated in hospital during the year was, males 392, females 55, and the ratio of deaths from phthisis to hospital cases from the same disease would be as 1 to 6.5 among the former and as 1 to 5.5 among the latter.

III. *Diseases of the Brain and Nervous System.*—There were ten deaths in this class, six among the men and four among the women, a comparatively large number among the latter. There does not appear to be anything very remarkable about the list. Four of the deaths were due to paralysis, two to apoplexy, and two to meningitis. There were 425 hospital cases among the men, and 77 among the women, including cases of insanity. An alteration in the mode of dealing with insane convicts is thus referred to by the Directors of Convict Prisons in their report :

"During the course of the year 1875 the practice of sending

male convicts who became lunatic to Broadmoor Asylum was permanently put an end to, and all male convicts who become insane during their sentence will in future be retained in a convict prison. Woking prison, which was constructed as a hospital prison, is charged with the care of these persons. The change was no doubt a judicious one in respect of the separation of men who were actual criminals undergoing sentence from those who had been acquitted on the ground that their crimes were to be attributed to their insanity, and who, therefore, might fairly be treated with a degree of indulgence which, so far as it is not a part of the medical treatment, would be inappropriate in the case of a convicted criminal; and there is good ground for anticipating that a propensity to sham insanity, or to that deliberate abandonment of self-control which resembles or may actually develop into insanity, will be checked by the knowledge that it will not lead to a transfer to the comforts and comparative freedom of Broadmoor Asylum."

The number of convicts in custody under treatment as insane at the end of 1875 was 187 (160 men and 27 women). Of these, 93 men and 26 women were at Broadmoor, the others at Millbank and Woking convict prisons.

IV. *Diseases of the Circulatory System.*—The heart and the larger vessels connected with it suffer from diseases which prove very fatal in proportion to the numbers affected. Out of 189 hospital cases there were 16 deaths. The Medical Officer of the male invalid prison at Woking says—

"The number in the prison so afflicted (*i.e.* with disease of the circulatory system) amounted to sixty, who were invalided for various organic diseases of the heart, irregular action, and aneurism of the large vessels, besides twenty-eight for varicose veins. The invalids suffering from such afflictions are often in want of medical assistance, and the fatal termination is no doubt retarded in many cases by the enforced order and regularity, as the patients suffering from such diseases appear peculiarly liable to fits of excitement."

V. *Diseases of the Respiratory System.*—There can be no doubt that convicts exposed on public works run considerable risk of suffering from acute attacks of inflammation either of the bronchial apparatus or of the lung tissue itself. And they also run the further risk of having repeated attacks of localised inflammations within the lungs; and the structural degeneration in these organs comes finally to reveal itself with the telling and irresistible force of pulmonary consumption. Hence it is that the conscientious prison surgeon ever has his stethoscope in hand and lends an apprehensive ear to the earlier signs of congestion and inflammation within the chest.

Bronchitis, pneumonia, and pleurisy show 461 out of a total of 561 hospital cases of diseases of the respiratory system, and they contribute twelve out of the fifteen deaths.

VI. *Diseases of the Digestive System* proved fatal in 10 cases (one being a female), and there were altogether 795 hospital cases (102 females). Diarrhœa required hospital treatment in 246 cases among the men. The regularity and moderation of the convicts' diet serve to protect the digestive organs from the pressure or strain arising from excess, and hence serious disease from mere functional derangement of these organs does not appear to be frequently met with.

VII. *Diseases of the Urinary and Generative Organs* were fatal in 9 cases among the men and in 1 among the women.

VIII and IX. *Diseases of Locomotory Organs, and of the Eye, Ear, &c.*, possess no special interest, in what is said of them in the returns.

X. *Diseases of the Cutaneous System*, although not fatal, are frequently a source of much pain and discomfort. Exposure to change of temperature, especially on public works, together with a probably vitiated state of blood, seems to cause a local determination to the skin very frequently in the form of boils, abscesses, and chilblains, in prisoners. Portland stands out prominently in this respect, and so does Chatham. We have always looked upon "ulcer" (which comes in this category) as fraught with suspicion among prisoners, unless, perhaps, it is associated with varicose veins.

As regards the more chronic or diffusely eruptive skin diseases, convicts are comparatively free. Psoriasis and eczema are the two that appear most frequently, but they do not often necessitate hospital treatment.

XI. *Accident, Injury, and Operation*.—The surgery of convict prisons does not take a very wide range. When we consider the contusions, the sprained backs, and the thousand and one odds and ends that constitute an accident sufficient to unfit the convict for his work and procure his admission into hospital, it will not be considered that 637 cases in this class are a very excessive contribution to the returns among the men. Portland itself contributes nearly one half, and the medical officer states that "the accidents were chiefly of a slight character, two fractured legs being the only cases of importance." How many of the "accidents" at convict prisons are "done on purpose" it would be impossible to say, but it is certainly satisfactory to find that the graver forms, at least, of self-mutilation are very seldom indeed resorted to. At Chatham, which had acquired an unenviable notoriety some years ago in this respect, the medical officer says—

"Malingering has assumed small proportions during the past twelve months. One prisoner sustained a compound fracture of the leg by placing it under a truck. The remaining cases of attempts at wilful injury, including the creation of artificial sores,

&c., amount to 34 in number, and, having being slight in their nature and extent, have quickly yielded to treatment."

It may be taken as exceptional and remarkable that no death resulted from accident during the year. That a large number of men, many of them altogether unused to manual labour, should be employed excavating, quarrying, building, &c., with no fatal and with but few cases of serious injury, betokens a commendable exercise of care and discretion on the part of those upon whom rest the more immediate supervision and responsibility.

XII. There were five cases of *Suicide*; all males. Hanging is the favourite mode of committing self-destruction among the convicts, and opportunity still appears to be given them of carrying out their rash purpose in their cells. Three out of the five occurred in this way—by suspension in their cells, and means might surely be adopted to deprive them of the facility of attaching the upper end of any strap or ligature at a height sufficient to effect their purpose. Three of the suicides took place at public-works prisons, viz. Portland, Chatham, and Borstal; a fourth occurred at Millbank, and the fifth at Parkhurst. The number is unusually large, as the proportion of suicides in the convict prisons taken over a series of years is really small. The Medical Officer at Portland "begs to mention as a curious fact that the case of suicide is the only one which has occurred in his experience during the sixteen years of his service" (in various convict prisons).

One of the cases illustrates the occasional anomalies of prison medical practice. If accidents are found to be at times "done on purpose," suicide would appear occasionally to be committed accidentally. The medical officer of Borstal prison states in his report, "One (prisoner) died from partial strangulation in his cell, he having tied portions of twisted rag tightly round his neck and loins concealed by his clothes. On being discovered, the circulation of his blood was obstructed, and though prompt measures were resorted to, they failed to restore the languid circulation, and he only lived six hours. The jury, in returning their verdict at the inquest, said that, under the circumstances, they did not consider it to be a suicidal attempt, but only for the purpose of evading labour." We are not told what the verdict was; but supposing the jury recorded it as "Death by misadventure," we are curious to know whether we would be at liberty to look upon convict "No. 68" as an experimenter who was anxious, despite anti-vivisection outcry, to test how far he could kill himself without totally extinguishing life, and as one therefore who died "all too soon" for science; or whether he is to be taken merely as another "sacrifice to that Moloch of Fashion, tight-lacing!" The man wished to evade labour. He did so, and died, as the doctor says, from partial stran-

gulation. If he did not commit suicide, then the convict prisons can only be credited with four deaths from self-destruction in 1875, instead of five.

XIII. *Anomalous cases.*—The great bulk of the hospital cases entered under this heading were due to “debility,” and a smaller number were cases under observation or of imposture and feigning. We look upon these cases of debility—there were 641 among the men and 140 among the women—as almost second to none in interest, for they are in large measure an indication of the extent of any pressure, undue or otherwise, exerted by the exactions of penal servitude upon men and women, healthy and more or less able-bodied. At the outset we take it for granted, indeed we believe, that convicts who bear the impress of constitutional or local disease or defect are not expected to perform the labour or task which penal servitude justly demands of the healthy and strong. And we are quite willing to accept it as a necessity of the case that a few originally healthy individuals should run down in their physical tone under the regime of a convict’s life, for, of course, there is a legitimate tear and wear of constitution in every human occupation.

A watchful eye, however, should be kept that these cases do not become numerous, and it is important that the meaning of this occurrence be borne in mind, not only as regards the individual, but also as regards the mass. What, as a rule, does “debility” mean in convict medical parlance? It means simply this, that a prisoner who, up to a certain point, was fit for hard labour, is now, *without any nameable disease being present*, unfit for it, and has either to be taken into hospital or put to work of a light description.

He may or may not have lost weight, perhaps he had not much to loose. But his muscles are soft and flaccid, his nervous energies have become relaxed, he is spiritless, and has not the heart, such as it was, for heavy and compulsory work.

At this point the risk lies in his susceptibility to the inroads of positive disease if he is driven beyond his strength, and the chain is apt to snap at the convict’s weakest point, his lungs. At this stage his salvation is quite possible, but it rests with the doctor, to whom the authorities give full discretionary powers for dealing with such cases. Upon the doctor, therefore, lies the onus of detecting possible mischief at the earliest moment, and of reducing or precluding the risk of the development of serious disease. And all this means constant watchfulness, repeated inspections, careful examinations on his part.

These reports are of much interest, and we regret having to forego the enumeration of many satisfactory practical points that are brought up in them.

They all point to a highly effective sanitary condition of the

Government prisons and to very favorable general circumstances under which a convict has to work out his sentence of penal servitude. A complete system has been organized under which the prisoner's capacity for labour can be utilised on the one hand, and under which his physical or mental defects, of whatever extent, can be accommodated by the needful relaxation or suspension of labour and discipline.

Of course there are, and ever will be, moot points, individual crotchets, and grievances, on one side or the other, on the matter of penal legislation and government; and we have not shrunk from mentioning possible risks that might suggest themselves from a medical point of view, especially where the balance of labour, diet, and physical condition are brought to an unusually fine point.

The great thing is to avoid extremes in such a question. Both are equally mischievous—that of sickly sentiment on the one hand, that of crushing dogmatism on the other.

In our opinion the authorities are desirous of doing what is right at once by the prison population and by the requirements of society.

Bibliographical Record.

Hammond and Winslow on Spiritualism.¹—The treatises of Drs. Hammond and Winslow before us refer to a phase of the human mind pregnant with dangers, if not disasters, to the community. It is worthy of grave thought that contemporaneously there should in this the nineteenth century be a popular though local belief in miracles performed at Lourdes (Dr. Capel's letter to the 'Times,' 18th September, 1876); in witchcraft, as declared in Warrington police court, where neither accusers nor witnesses could be compelled to attend from fear of the power and incantations of an octogenarian woman ('Times,' 16th September, 1876); and in spiritualism, asserted to possess ten million votaries and ten thousand mental victims in the United States (Dr. L. S. F. Winslow, on 'Spiritualist Madness,' 1877). It is suspected that this latter calculation is an exaggeration dictated by alarm or philosophical antagonism and party spirit, as it is not countenanced by any statements in Dr. Hammond's work nor borne out by the statistics of American asylums. Thus, in four of the largest of these, in the reports of which the causes of 17,060 cases of insanity have been recorded, not more than 228 are attributed to religious excitement and spiritualism, no distinction being attempted between the nature and the number of the patients thus tabulated. Further, in six other institutions, where the gross number of patients received during the past two years amounted to 1394, only nine were recorded as originating in spiritualism and religious excitement. That the influence of spiritualism and genuine piety or even religious fervour should be confounded etiologically is perplexing, but the confusion suggests the painful reflection that the dogmata and the experience of professors and proselytes of spiritualism have been accepted as truth by persons of unquestioned orthodoxy, have been by them incorporated, as it were, with their Christian creed, and elevated into the rank of

¹ 1. *Spiritualism and allied Causes and Conditions of Nervous Derangement.* By WILLIAM A. HAMMOND, Professor of Diseases of the Mind and Nervous System in the Medical Department of the University of the City of New York, &c. London, 1876.

2. *Spiritualistic Madness.* By L. S. FORBES WINSLOW, M.B. Cantab., D.C.L. Oxon., Lecturer on Mental Diseases, Charing Cross Hospital, &c. London, 1877.

confirmations of the history, miracles, and omnipotent influence of the truths once delivered to the saints, and, finally, have assumed the position and power of a new religion in the judgment of others who doubt and reject the faith of their forefathers. Indeed, under the belief in spiritualism the prophets, the apostles, the martyrs, have been summoned to give evidence in modern and common-place matters, and dialogues have been reported as sustained with them as to truths and mysteries which they lived or died to establish or vindicate.

This is not intended as a protest or a crusade against a new heresy; in fact, we feel less interest in the fanatical or pietistic aspect of this craving for the marvellous than in the acceptance of imperfectly supported or utterly absurd propositions by the wise and the learned and the scientific. It is startling to find that there must be assorted together in the consciousness of such minds associated but distinct and apparently contradictory and repellant bundles of conceptions, which in the one case are held tenaciously by all mankind, and in the other are merely tolerated as possible by the credulous, the illogical, or the susceptible, and rest their credibility and possibility upon evidence which would be repudiated in the courts of law, theology, and medicine, as well as those of common sense.

It must not be supposed for a moment that we regard all the subjects treated in these volumes as of the same importance or credibility, that we are sceptical as to the connection of truth with error in many of them, or that we acquiesce implicitly in the thaumaturgic zeal of Dr. Winslow or the cold doubt or indifference of Dr. Hammond. We would recoil from confounding curing mediums with the impressions of Bernadotte of Lourdes, and from declaring that there was no truth nor resting-place in the experiences of individuals morbidly affected; but heterogeneous subjects, from legerdemain to stigmatisation, are so herded and huddled together in the voluminous treatise of the latter physician that we are compelled to speak in general terms, and to condemn rather indiscriminately what is open to suspicion, even where it does not call for condemnation. It is unfortunate that Dr. Hammond, an eminent and universally respected physician, should have announced, as one of the pivots upon which his observations might be expected to revolve, "the outpouring of the Spirit of God," an expression which would be blasphemous if it were not the fruit of ignorance, and one, to the physician's perception, often only another name for epilepsy, chorea, catalepsy, ecstasy, &c." (Preface). However, interpreting these words as rashly spoken, and as meaning nothing more than that certain physical and mental conditions known to depend upon detectable disease are or may be attributed to the direct result of Divine interposition, we shall treat his repertory of marvellous or inexplicable conditions or

events more in its psychological than in its controversial or ethical aspect, and chiefly as these conditions or events implicate mental health.

It is probable that the present advent of this publication may be so far due to the general if not universal curiosity, coquetting, patronage, with which spiritualism in its new birth has been received, by those who went to scoff as well as by those who remained to believe, but it is attributed by the author to the demand for a new edition of a brochure, itself the evolution of an article in the 'North American Review.' This growth has now attained goodly proportions and pretensions, and is in its present form endowed with all the temptations and luxuries of broad margins, a beautiful type, illustrations, and attractive contents. We would add to these characteristics that the author has bestowed great care, and even learned research, upon the elaboration of his materials, and, although evidently influenced by a preconceived bias or theory, and viewing facts through a dark or distorted medium, he is above all suspicion of prejudice. He advances as a universal solvent of what he stigmatises as "abnormal conditions," that they are "material derangements of the organs or functions of the system," and that, as a corollary of such explanation, we no longer "believe that the bodies of lunatics, epileptics, and hysterical women are inhabited by devils and demons."

Now, it is notorious, although demonomania and demonolatry may occasionally be exorcised by galbanum or galvanism, that were the absence of evil spirits or spirits of any kind to depend upon the demonstration of structural change in the bodies of the possessed, the conclusion would be lamentably different. In many of the class of patients, hystero-epileptics, to which his remarks apply, no pathological alterations whatever can be detected, and we can recollect—in examining all the organs of some of the last victims of the Leaping or Forfarshire ague, who approached more nearly to the Convulsionnaires and cestatics of mediæval times than other hysterics or choreaics,—the astonishment of all engaged, on discovering the perfect soundness and integrity of all the viscera and tissues. In the lack of such visible morbid disorganization, and dismissing the archæus from his supposed duties in the stomach, Dr. Hammond refers to another imponderable, "animal electricity," which he affirms to be identical with that developed outside the body, as likewise innocent of the production of any form of spiritualism, though calculated to inspire wonder and fear, and cites the discovery of Reichenbach as to the influence of magnets of great power in exciting peculiar sensations in people of nervous constitution (calculated to amount to one third of the population), and of enabling them to perceive the luminous flames said to have been given off from the poles, as to a certain extent explaining the claims of mesmerism. While limiting the higher manifestations of mind to the brain, the author believes that wherever

there is grey nervous matter in the spinal cord and sympathetic system there nerve power is generated, and "these parts are capable of originating certain kinds of mental influence, which, when the brain is quiescent, may be wonderfully intensified," upon which may be said to hinge Hartman's 'Philosophy of the Unconscious,' a concession which we suspect involves more than archæus, transferred sensibility, clairvoyance, presentiment, and many other of the preternatural gifts of magnetisers.

In a section on the effects of concentrated attention Dr. Hammond writes—

"Thus, a lady who has been under my professional care for intense nervous headaches, and who is of a very impressionable organization, is able at will to produce a pain in any part of her body by steadily fixing her attention upon it,"

and corroborates the experience of others who have traced organic diseases of the heart, stomach, and so on, even in muscular tissues, to intense attention or emotion. In considering the power of subjective states, of conditions of consciousness, of hope or fear, in suspending or promoting nutrition, or otherwise affecting structural integrity, it is to us more difficult to conceive the influence thus exercised as residing in the tissues than to attribute it to an external, a supernatural, or a Divine origin. But keeping before us these facts, we are surprised that Dr. Hammond should deal so harshly and sceptically with the 'stigmatics,' and regard the local sanguinary sweating as the product of religious imposture rather than as a pathological result of intense and ecstastic feeling and localised attention; although in some instances he admits that the local hæmorrhage may depend upon purpura, syphilis, or diseases of the skin. Of this class about 160 examples are alluded to, ranging from St. Francis of Assisi to Louise Lateau, the most celebrated in modern times. Only one of this numerous host was of American birth. The Belgian prodigy was subjected to severe scientific scrutiny, and according to her medical biographer—

"The blood was natural except that it contained an excessive amount of white corpuscles; the air she expired contained the vapour of water and carbonic acid; her weight had not materially altered since she had come under observation; she consumes carbon, and it is not from her own body that she gets it. Where does she get it from?"

But the caustic critic answers—

"She eats, and that were she as vigilantly watched as other fasting girls she would die."

Under a cognate category is placed levitation, or rising and floating in the air, as first observed among saintly personages, and latterly among those who claim similar, though not saintly, attributes,

and, whatever may be thought of the former, the latter are according to Dr. Hammond charlatans or conjurors. Of those who were poised above the earth by some unknown intrinsic power, or in proof of the divine favour, about twenty-five are named, including St. Ignatius, St. Dominic, St. Teresa, and others of world-wide celebrity and Catholic sanctity. After insisting upon the possibility of favoured or ingenious individuals lengthening or shortening their bodies at will, and upon those extravagancies of *cœnæsthesis* during which the members seem to swell or diminish in size, Dr. Hammond abruptly cuts the gordian knot of difficulties connected with the subject by asserting of both ancient and modern levitators that there "existed hallucination on the part of the subject characterised by the sensation of rising in the air or of flying, and illusion on the part of those asserting themselves to have been witnesses." We contend that this is rather a sharp and sweeping judgment where such "clouds of witnesses," neither in wine nor in a straight jacket, testify to something new, extraordinary, or anomalous in what they saw or believed that they saw. In place of so wholesale a condemnation, could not an analysis have been attempted of that appetency for tidings and trials and testimonies of the unseen and hitherto unknowable psychical universe so epidemically prevalent in an age called, perhaps calumniated, as one of infidelity, doubt, and despair? That such a craving should effect in some shape or other a large minority of men of British origin, that it should be intertwined with even scepticism itself, and held with a tenacity almost unexampled, are phases of thought, or imagination, in themselves worthy of investigation. The critical point here is that, not merely does no antagonism or antipathy in consciousness exist to the sincere and strong belief, but that there is a faculty or feeling coeval with the growth and maturity of reason which creates and compels a belief in the marvellous, in that of which there is no ordinary proof, and against which the laws of logic and science protest. Can this congenital instinct or intuition be granted merely to betray, to darken, and destroy mental perspicacity? It is true that there have been martyrdoms in support of every creed, of "trifles light as air" as well as of "truths of Holy Writ," but in the present case the credulous are condemned to that worst of martyrdoms, ridicule, contempt, contumely. Their belief is certainly worthy of respect except when it outrages decency and decorum. Were the explanation of those who have imbibed advanced views of spiritualism accepted, this sincerity and self-abnegation would be referred to the supernal influence added to that of the natural man; as in speaking mediums the organs of articulation are set in motion independently of the will of the speaker by the double or supplementary agent, to reveal the past, to predict the future, to utter languages, even to the number of seven, previously unknown and

still unintelligible to the speaker. This gift is said to be shared by lower animals. In respect to the pneumographic or writing mediums, the hand is under the control of a similar guiding or over-mastering impulse; through the instrumentality of seeing and auditive mediums visions and voices from and around the eternal silence reach the passive but cognisant mind.

There have been healing mediums perhaps from the beginning of time, certainly through all Christian ages, down to the omnipotence of royal touch, and they still abound. Those who can recall the triumphs of mesmeric passes and hypnotism in the subjugation of the external senses of pain under the surgeon's knife, and of the will, not merely in the abolition of personal volition, but in the substitution of another and distinct will for that of the patient acted upon, can understand that such relations between human beings are not altogether vain or visionary. With the experience of Dr. Hammond in relieving neuralgia by the touch of a thermometer, and the wonders related under the designation of hysteroid affections, it need create no astonishment that he admits the reality of thousands of cures by aid of the waters of Lourdes. These miraculous effects testified to by Dr. Capel ('Times,' 18th September, 1876) on the authority of Dr. Dogous, an eminent resident practitioner and medical assistant of the court of justice, can of course be attributed to the potency of the imagination. This is the substitution of a word for a thing, of an abstract and undefined mental process for obscure and inexplicable physical changes. Who can assert that the restoration of hearing, the removal of paralysis, and so forth, are not the legitimate signs of that local volition and intention and instinct as seen in the growth and reparation of tissues, &c., over which neither intellectual will, nor imagination, nor creative hope can or do exercise any control? But, further, it is known that the tact, talent, reputation, of certain physicians impart efficacy to their medicines, success to the practice, and that the supposed power of the prescriber enters largely as a curative element into his prescriptions. Is it, then, more difficult to conceive that recovery may flow from the benign interference, through a drop of water, of a Being known and trusted as possessing the remedial powers, than that it emanates from trust in the interference of a fellow-mortal known to possess no such remedial power, known as our equal, friend, our inferior?

In the highest and holiest popular movement, in the propagation of truth itself, there will doubtless insidiously mingle monstrous credulity, cunning and selfish deceit, and it may be expedient to rely upon partial and imperfect exposures and explanations of spiritualism in the absence of any others and until final scientific demonstration can be reached; but it appears premature to adduce sleight of hand, modern necromancy, suggestion, unconscious muscular contraction or fibrillation, assumed inspiration, humbug,

even fanciful, or, at all events, hypothetical functions of portions of the nervous system, for removing all difficulties, as has been done in this volume, especially as its author himself confesses that we are too ignorant of the physiology of that system to pronounce categorically upon the extent and phenomena which may be forestated; and we recognise sound prudence and philosophy to consist in confessing with a distinguished physiologist that there are many facts and features in spiritualism inexplicable upon known scientific principles, and in remitting the whole subject for further and more enlightened investigation.

Dr. Winslow's pamphlet is devoted to an earnest protest against the encroachments of spiritualism upon mental equilibrium and equanimity, to an identification of the evils attending the spread of such a form of false belief with the epidemic madresses of past ages, and with fanaticism, superstition, excitement, and error, wherever they now prevail; and, lastly, in pointing to the sources of safety—sound sense, and morality. Certain proceedings in this country may have suggested the opportuneness of such a publication, but the author affirms that he was mainly instigated to venture upon his present appeal by the ravages committed on healthy mentalisation by this cause in America, and by events which came under his personal knowledge during a recent visit to France. Of his experience on the latter point he details six cases of insanity traceable to some modification of animal magnetism, characterised by the perusal of what may be called the literature of spiritualism, conceptions of exalted powers, of the influences of Divine or diabolic agencies, delusions, delirium, violence, and other symptoms common to the advanced stages of all forms of mental disease. It is probable that the coloration given to such cases amounted to nothing more than what is imparted in successive ages by predominating opinion, whether it be true or false, an illustration of which is afforded by Esquirol's remark that among madmen the fear of the police succeeded fear of the Jesuits; but it is wise to signalise the development of a new element in the pathogeny of moral contagion.

These pages come from the hands of a psychologist, and contain much interesting and even romantic matter, but are couched in a popular and ethical style, which will render them acceptable, and, it may be, useful to those who doubt as well as to those who believe.

Sanné, Treatise on Diphtheria.¹—The reasons for undertaking this voluminous work, and the peculiar circumstances under which Dr. Sanné has contemplated and completed the task, are satisfactorily put forward by the author in the introductory pages.

¹ *Traité de la Diphthérie.* Par A. SANNÉ. Pp. 654. Paris, 1877.

Since the appearance of Bretonneau's well-known 'Memoirs on Diphthérite,' many investigations have been made and many publications on the subject have appeared, among the most distinguished of which are those of Trousseau, who added to the picture of the disease several accessory features which had escaped his predecessor's notice. The theories advanced by these two distinguished physicians have stood the test of time, and in the meanwhile diphtheria has continued its march and has extended over nearly the whole world. The study of its characters has been pursued with perseverance, new views have been promulgated, and some of its most important phenomena have been carefully re-examined, even if they have not yet been completely explained. It was necessary to collect together these scattered elements of science and to classify them, to determine the progress which had been made, to explain the present state of information as to points still under discussion, to indicate the results of personal researches, and, finally, to present diphtheria in its complete and entire aspect.

This task Dr. Sanné states that he has undertaken, having felt himself irresistibly attracted towards it, inasmuch as his studies had been directed for a long time to the subject of diphtheria during his pupilage to Trousseau and Barthez, who enabled him to observe closely the cases under their care. In 1869 he investigated particularly a somewhat restricted branch of the subject, namely, the results of tracheotomy, but the numerous materials then presented to his notice, though only slightly recorded, have since been multiplied by an imposing number of cases taken by himself at the Hospital of Sainte-Eugénie, and occurring in his private practice. In addition to these sources of information he has made use of the notes taken of all the patients attacked with diphtheria in Barthez's wards from 1869 to 1874, and of all those in the same physician's wards from 1854 to 1869, and thus his present work is founded on about fifteen hundred cases from his own observation, as well as on several memoirs and treatises published by others. He has also endeavoured to study the disease in historical documents both in France and in other countries, and to give a general abridgment of the principal epidemics which have broken out in the old and the new world.

Those who know the difficulty of defining precisely what is meant by the term diphtheria as distinguished from other allied affections, and who are also aware of the mysterious and capricious course very often taken by the malady now so called, will be able to appreciate the magnitude of the labour undertaken by Dr. Sanné, the toil being infinitely increased by the different names under which the same disease is described by various writers in ancient times. We might advert, as additional sources of an author's difficulty, to the imperfect or erroneous descriptions given of the causes of death in public documents, as seen even under our own much-improved system

of registration, in which, from the hurry of medical practice, the neglect or the prohibition of post-mortem examinations, and other causes, the names of disease are too often given in such a loose and general manner as to render them almost useless as scientific data. In addition to all the causes of perplexity just mentioned, it must be admitted that the visitations of diphtheria, especially in its epidemic form, have sometimes been few and far between, so that for many decades, or even perhaps for centuries, the disease has been absent from certain localities, where it may nevertheless reappear suddenly with all the features of a new and unknown visitation. It is quite within the limits of truth to state that, when the epidemic of diphtheria first appeared in the vicinity of London and in the metropolis itself, about the year 1857, hardly any of the then existing race of practitioners had ever seen such an epidemic before, and while some regarded it as a new malady, others disbelieved its existence altogether. None of the established books on British medicine of that day contained any accurate description of its features, nor was the disease recognised in the hospitals or specially alluded to by the teachers in the medical schools. All that was known of it was from the descriptions of Bretonneau, which were familiar to comparatively few practitioners in this country, of whom, however, one or two had seen the disease with Bretonneau himself at Tours. Since 1857 or 1858 diphtheria, as an epidemic, has entirely disappeared from certain localities in England where it raged extensively, and, on the other hand, it has appeared in other districts which were formerly exempt, so that it is quite probable, and indeed it is certain, that many practitioners in the present day have never witnessed an epidemic. Whether diphtheria, however, is necessarily only epidemic or whether it is not always more or less present among us, is a different question, on which Dr. Sanné's treatise throws considerable light.

The problem as to the real nature of diphtheria and its relations to other diseases cannot possibly be solved without a very extensive acquaintance, not only with the clinical features of the malady, but with the literature of the subject as it is presented in a multitude of scattered monographs. It is quite idle to suppose that diphtheria is a new disease, such a supposition being contrary to all medical experience; and if it be not a new one, then of course it has prevailed, like other maladies, from the earliest period of recorded history. Bretonneau, among his other merits, successfully proved that the descriptions of many medical writers, particularly the Spanish and Italian, clearly denoted the existence of diphtheria as a destructive epidemic in the seventeenth and eighteenth centuries, and at later periods it is equally certain that epidemics of the same nature, though often separated at long intervals, have been observed in Sweden, Germany, the American continent, and even in some parts of Great Britain.

The epidemic which broke out at Tours and its vicinity about the year 1818 first directed Bretonneau's attention to the subject, and it is to be remarked, as a proof of the capricious nature of the disease, that Bretonneau himself, before that period, had never seen a case of the kind, nor had any of his fellow-practitioners, so far as he was aware. It is, therefore, almost needless to observe that, in presence of so mysterious and unusual a malady, Bretonneau was puzzled at first as to its real nature, and hence his earlier memoirs contain many views and speculations which subsequent observations and experience have proved to be erroneous.

Dr. Sanné, however, does no more than mere justice to Bretonneau in pointing out the valuable nature of that physician's researches, which have certainly directed the course of all subsequent investigations into the nature, the history, and the treatment of diphtheria. Dr. Sanné takes up the historical thread where it was left by Bretonneau and Trousseau, and thus he not only traces the course of the malady from the dawn of written language, when all descriptions have necessarily an element of obscurity, but he pursues the inquiry down to the present day, when diphtheria has become a household word, and almost every country has at one time or other experienced its ravages. If Dr. Sanné shows himself rather imperfectly acquainted with some recent epidemics of diphtheria, his excuse may well be pleaded from the multiplicity of the materials to be consulted; and it must be admitted that, even if he had not travelled in his researches beyond the limits of France, the sources of information are sufficiently abundant to enable him to write with authority upon the subject, the more especially when his literary labours are supplemented by such large practical experience as he has been fortunately enabled to acquire.

It is perhaps necessary to remark that the subject of diphtheria, in all its bearings, is still imperfectly understood, and this result is in a great measure due to the want of some intelligible clue to guide the mind through the labyrinth of speculation and theory in which the truth still lies in a great measure concealed. This clue Dr. Sanné appears to think he has discovered, and it is *the false membrane*. The characters of this false membrane are, of course, fully described in his pages and elsewhere. It may have its seat in various parts of the body, yet it is by no means to be regarded as a mere local affection, but the manifestation of a general and septic disease. What the cutaneous eruption is to scarlatina, and what the lesions of Peyer's patches are to typhoid fever, so is the false membrane to diphtheria. The morbid conditions in the last disease, in Dr. Sanné own words, "comprise *the fundamental or primitive lesions*, namely, the false membrane and the alterations of the tissues underlying it. This is the general pathological anatomy of diphtheria." The second class of lesions are those which, besides the

false membrane, are met with in the organs and tissues of subjects attacked by diphtheria, and are attributable to the influence of that disease. These are the *secondary lesions*.

The false membrane may be limited in its extent, and may give rise to only trifling inconvenience in some cases, but nevertheless there is always more or less constitutional disturbance; in other cases, on the other hand, its mere presence may be fatal, as when it obstructs the air-passages. The microscopical characters of the false membrane may vary in some measure according to the nature of the structures on which it is developed; thus, it may present certain varieties respectively on the fauces, in the larynx, in the trachea, and in the large bronchi. Dr. Sanné agrees with those authorities who consider the existence of the diphtheric membrane to be limited to the parts of the mucous membrane which are near the great outlets of the body, or to the external cutaneous surface; and he therefore entirely repudiates the views of those German authors who apply the word "diphtheritis" to such diseases as typhoid fever, cholera, dysentery, metritis, &c.

Here we must remark that the application of the words "croup" and "diphtheritis" by Virchow and some other authors to diseases with which those words have no etymological or any other relation is unfortunate, and certainly constitutes an element of confusion. "Croup," if it means anything at all, certainly refers to some noisy sound made in breathing or coughing, and the word "diphtheritis" clearly denotes some disease characterised by the presence of a pellicle or false membrane. How then these words can be legitimately applied to such diseases as pneumonia, or nephritis, or dysentery, or hospital gangrene, it is difficult to conceive, and Dr. Sanné energetically protests against such a misuse of terms. He therefore strictly confines the terms "diphtheria" and "croup" to the sense in which they were originally employed, the latter by Home and the former by Bretonneau.

Like nearly all other French writers, Dr. Sanné regards the "croup" of Dr. Home as the tracheal diphtheria of Bretonneau, and he gives abundant reasons for so regarding it, not the least cogent of which is that Bretonneau himself decidedly took that view, and indeed censured Home for failing to see in his "croup" the representative of the suffocating and fatal epidemics described as occurring in Spain and Italy in the seventeenth and eighteenth centuries. Dr. Sanné strongly maintains, then, that croup, or rather true croup, is neither more nor less than tracheal diphtheria, and, indeed, he divides diphtheria into *diphtheritic angina*, when the disease is above the glottis, and *croup*, when it is below it, the two being identical in character and differing only in their seat. He considers that the malady usually called croup by the public is only stridulous laryngitis, and is comparatively mild: and

that, anatomically, the false membrane is the only real diagnostic sign. As to the view sometimes expressed, that there are two kinds of croup, namely, a diphtheritic and a non-diphtheritic, he regards it as quite theoretical and destitute of foundation; in his opinion, the presence of false membrane in the larynx and trachea plainly denotes the disease known and described as "true croup," and this is the same as laryngo-tracheal diphtheria. He agrees with Bretonneau, Trousseau, and others, in considering that in the great majority of cases of "croup" the local manifestations commence on the fauces, and that when these characters are not observed the examination has been insufficiently conducted. But he also points out as a source of error, even under the most careful search, that the false membrane, as he has found in post-mortem inspections, is occasionally placed behind the tonsils and is therefore out of sight during life. He admits, however, that the local phenomena, in a certain number of instances, really begin in the larynx and trachea, a condition which is well known in France, where it is termed the "*croup d'emblée*," an expression very difficult to render by any exactly corresponding English words, but meaning "coming on all at once."

It is quite impossible for us to follow Dr. Sanné at length through his interesting volume, and it must suffice to state generally that he has considered the subject of diphtheria in all its aspects, classifying the varieties of the disease according to the organs and tissues attacked, as for instance oculo-palpebral diphtheria, diphtheritic otitis, cutaneous diphtheria, &c. The section on diagnosis lays down the distinctive characters separating diphtheria from other diseases which might be, and indeed are, often mistaken for it or confounded with it, such as scarlatina, follicular tonsillitis, infantile laryngitis, aphthæ, ulcero-membranous stomatitis, &c. The chapter on etiology comprises an historical survey of the principal epidemics which have prevailed in France and elsewhere since the year 1862, but this part of the subject is very imperfectly treated, especially as regards other countries; and it is followed by an interesting discussion as to the different modes in which diphtheria originates or is transmitted from one person to another. The contagious character of the disease is universally admitted, though the experimental investigation of this question presents some perplexing contradictions; and it would seem that children are far more liable to the infection than adults. With regard to locality, although it is generally conceded that want, misery, and insanitary conditions favour the development of diphtheria, it is but too well known that the rich and well-to-do classes are nearly as often attacked as the very poor. Dr. Sanné, indeed, states that the great majority of patients attacked with diphtheria are seen in the French hospitals and belong to the poorer classes, but we doubt whether the same can be stated in reference to London or other large English

cities, for, unless we are much mistaken, diphtheria is by no means a very common disease in our hospitals, and is chiefly met with in private practice and in localities where its presence is often little anticipated.

The treatment of diphtheria is very elaborately and ably handled, and occupies more than 250 pages of the book. It is, we think, a great merit in Dr. Sanné that he does not lay down any dogmatic therapeutical rules or pretend to have discovered any infallible cure. He points out that many of the cases, where only the fauces are affected, are slight in their character, and might, and probably do, recover under any treatment or without any treatment at all; while, on the other hand, some are so dangerous as to defy all the appliances of the healing art. He divides this part of the subject into the Medical and the Surgical portion. The former includes the application of local remedies to the affected parts, the administration of internal medicines, and the appropriate dietetic management of the patients; the latter is all included under the head of Tracheotomy. This important operation, which in many of the worst cases is the only resource, and that a dangerous one, has lately been far more successful than it was in former years. It was one of the greatest achievements of Bretonneau to perform this operation with fortunate results after he himself had previously failed, and it is gratifying to find that both in France and England it has been the means in the present day of rescuing many patients from what appeared to be certain death.

Piffard on Diseases of the Skin.¹—This handsomely printed volume of 375 pages is described by the author as an elementary treatise on diseases of the skin. It can scarcely be recommended as a text-book, on account of the very unequal manner in which it is written. Certain chapters are devoted to the discussion of the pathology and treatment of some of the more prominent diseases which seem especially to have attracted Dr. Piffard's attention. These are well written and contain a number of suggestive statements. But the intermediate portions of the book are occupied with meagrely written chapters of the kind known to bookmakers as padding. One almost rivals the celebrated chapter on the snakes in Norway; we quote the entire:

“CHAPTER XVII.

“*General Non-Diathetic Affections.*

“The affections embraced in this class are well and fully con-

¹ *An Elementary Treatise on Diseases of the Skin, for the use of Students and Practitioners.* By HENRY G. PIFFARD, A.M., M.D., Professor of Dermatology, University of the City of New York; Surgeon to the Charity Hospital, to the New York Dispensary for Diseases of the Skin, &c. &c. With illustrations. London and New York, 1876. Pp. 375.

sidered in treatises on general medicine. They usually come under the care of the general practitioner, not of the specialist. For these reasons they will not be considered in this place."

Urticaria, its varieties, ætiology, diagnosis and treatment, is summarily disposed of in a chapter of about eighty lines, very barren of information and largely consisting of negative statements, such as, "as far as I am aware, no microscopical examination of the wheals has ever been made;" "in many patients the uterine, ovarian, and digestive apparatus are in perfect order, and careful supervision of the diet fails to reveal the exciting cause;" "the treatment of chronic urticaria is very unsatisfactory." Writing of this kind benefits nobody. The well-informed reader who has turned to the book for original observations will be repelled by pages of meagre compilation interpolated between the more genuine portions of the book, and the student who adopts the work as his guide will not unnaturally be disappointed to find the information in this imposing octavo volume less precise and copious on many points than in some of the humbler students' manuals in vogue. Writers on skin diseases should bear in mind that we are already overdone with treatises, systems, and manuals. For the present we want no more books repeating what has already been said over and over again in other books, but individual experience and observations should be presented in a compact form, unaccompanied by needless repetition of the common knowledge. Had Dr. Piffard confined himself to narrating his own observations and supporting those doctrines in which he believes, his book would be very considerably smaller and much more valuable than it is at present. Dr. Piffard is a follower of the French school and gives his adherence more especially to the doctrines of Hardy. The chapter on the scrofulides is confessedly based on the French observer's well-known monograph on the subject. He considers lupus a bad term, inasmuch as it was originally used by Bateman and Willan to designate a tubercular lesion of the skin, and it does not include those affections allied to tubercular lupus in which the initial lesion consists of pustules, scales, or erythematous patches. Excluding, of course, those syphilitic affections which might be mistaken for it, he believes we shall do wiser to cast aside the term lupus and to give the general name of *scrofulide* to all those diseases which are developed under the influence of scrofula. This is, we need scarcely remark, begging the question. It is by no means generally admitted, certainly not in England or Germany, that lupus is a manifestation of scrofula, and until the evidence is complete either one way or the other we should do very unwisely to discard a name which binds us to one theory for one which carries with it a very sweeping pathological generalisation. Few of our readers, we should say, will concur with the author in ranking the affection known as rodent ulcer as a *scrofulide*.

The most interesting portion of the work, however, is that devoted to our old friends the "dartres," rechristened with the uncouth name of "rheumides." In the existence of the dartrous or rheumic diathesis Dr. Piffard is a firm believer, although he admits that from the very nature of the case an absolute demonstration is impossible. His views are "derived from observation, study, and experiment," and agree with the opinions of the later French writers in attributing eczema, psoriasis, and pityriasis, to the retention of some *materies peccans* in the blood. He says—

"The affections pertaining to this diathesis are in all probability due to the accumulation in the blood of an excess of certain excrementitious substances, and presumably those which are also efficient in the cause of gout and rheumatism, with perhaps the addition of a few others whose relations to morbid conditions have not as yet received much attention. Although it is far from being susceptible of demonstrative proof, it is more or less probable that the following are the noxious agents; namely, uric acid, lactic acid, oxalic acid, creatin, creatinin, and possibly others. The first, fourth, and fifth of these are always derived from pre-existing albuminoid substances; the other two, sometimes from albuminoids and sometimes from substances belonging to the amylaceous and saccharine groups, and all of them represent either steps or side-products of the processes which bring about the metamorphosis of food into tissue and that again into substances ready for excretion."

The immediate consequence of the blood being surcharged with these morbid materials is an increased demand for excretion:

"This duty the kidneys will perform up to a certain point and for a certain time, but at last, failing to be completely removed, they seek other channels of exit, chiefly the bowels, but in part also the skin. The bowels, being accustomed to the office of depuration, do not complain when any slight demand is made upon them, but the skin, being less accustomed to the performance of this function, exhibits its impatience by *pruritus* and its rebellion by *eruption*."

The conditions requiring treatment in a case of "rheumide" disease are (a) blood surcharged with insufficiently oxidized excrementitious principles less soluble than urea; (b) sub-alkaline blood. The conditions may be produced by (a) deficient renal action, or (b) hyperproduction, (c) over-supply of albuminoid food, or (d) failure of the oxidizing processes to fully complete the normal changes. We are therefore not surprised to find Dr. Piffard relying greatly on purgatives, diuretics, and diaphoretics. He praises especially the Turkish bath, which in our hands has proved entirely unsuccessful in cases of the "rheumides." Although we cannot altogether agree with the humoral theory we have endeavoured to epitomise above, we have found Dr. Piffard's chapters on this division of the subject eminently readable and suggestive. The volume is copiously illus-

trated. The microscopic drawings are for the most part taken from Neumann's book. Others are photographs from actual sections. These, though they testify to the industry of the author, are lacking in definition and quite unserviceable.

Fieber's Surgical Observations.¹—This pamphlet is founded on observations made during the Austro-Italian campaign of 1866, chiefly after the battle of Custoza. Since that time, to judge from the number of authors quoted, Dr. Fieber has read extensively on the subject of military surgery, and has now embodied in this work the results of his combined study and experience.

He commences by excusing himself for presenting no statistical tables by a general assertion that statistics are unreliable, and consequently more or less useless. There can be no doubt that there is much truth in what he says, for the sources of error are numerous, and in most cases unavoidable. The number of wounded after a great battle must make it impossible for any notes of scientific value to be taken for many days, and in the hurry of those first few days inaccurate diagnoses must be far from infrequent. The necessities of warfare almost invariably oblige the frequent removal of patients from one place to another, and the surgeon who commences the treatment of a case is rarely able to follow it to its conclusion.

The greater the number of wounded and the more extensive the statistics, the more likely are these errors to creep in. The circumstances under which the wounded are placed have, moreover, so great an influence on the results of the treatment as to render the statistics of one war useless for comparison with those of another. Thus the French, before the battle of Sedan, had spent many days on short and bad rations, and it was hardly to be expected that men already exhausted when they received their wounds should progress as well as others who were comparatively fresh and well fed. Under such circumstances conservative surgery might easily fall into disrepute if statistics were to be relied upon as conclusive of its value. In the same way the statistics derived from the Crimean war, during which the troops suffered severely from want of food, scurvy, and cold, would be useless for purposes of comparison with those obtained from the perfectly organised hospitals under the charge of Stromeier during the various German wars of the last fifteen years. It is evident that, if guided by statistics alone, a surgeon might err grievously in the treatment of his patients. With exhausted and underfed troops, in foul and overcrowded hospitals, conservative surgery could only lead to fatal results, however favorable the statistics of more fortunately placed surgeons may be.

¹ *Chirurgische Studien und Erfahrungen*. Dr. CARL FIEBER. Wien, 1875.

The wounded who came under the observation of Dr. Fieber were placed in three temporary hospitals. The first of these was a large orphan asylum for girls, and contained 200 beds; the second was a spacious garden, in which forty-eight tents were erected, accommodating 384 patients; and the third was a large church, in which from 90 to 100 beds were placed. Opportunity was, therefore, given of comparing these three forms of housing wounded, and estimating their comparative advantages and disadvantages. Dr. Fieber decides unhesitatingly in favour of tents as the healthiest, in this confirming the views of many others who have discussed this question. But although tents have the great advantage of securing fresh air and all its blessings to the patients lodged in them, yet there are many drawbacks to their use. The subdivision of the wounded into so many small groups, comparatively isolated one from another, necessarily adds greatly to the difficulty of providing proper nursing and attendance, and it is seldom that a sufficient staff is available for such purposes. It usually happens, therefore, that at the earliest possible opportunity the wounded are removed from the tents and placed in such buildings as schools, convents, barracks, &c. The transport of tents is, moreover, a considerable encumbrance to an army, and forms another objection to their use. The tents in which Dr. Fieber's patients were placed presented, in addition, another serious fault. They were not specially intended for hospitals, but were merely conical sail-cloth tents with a round base, and seven or eight yards across. The sloping sides rendered the dressing of the patient somewhat difficult without moving him first into the middle of the space between the beds. Many also were not waterproof, and the heavy thunder-showers of July and August occasionally soaked the miserable occupants. Dr. Fieber attributes the occurrence of a case of tetanus to this cause. With all these drawbacks, however, he prefers tents when possible to extemporised hospitals in permanent buildings. To churches he strongly objects. He complains that the "dim religious light" depresses the patient; the coldness and draughtiness expose him to many dangers, and the height and smallness of the windows make proper ventilation impossible.

All other buildings, such as schools, barracks, convents, and private houses, soon become foul. The experience of other writers has shown that such buildings, in spite of all care, become unhealthy towards the end of the second week. Patients who have undergone primary amputations are by that time far on the road to recovery, and frequently escape the pernicious effects of the impure atmosphere; but the mortality amongst the secondary operations, in which the fresh wound is exposed to the air already contaminated by the effluvia from numerous wounds, is always great—sometimes fearfully so. The author seems to have had no experience of

the wooden huts, which were found so advantageous during the Franco-German war.

As usual in military practice pyæmia was by far the most frequent cause of death. Dr. Fieber has nothing new to communicate on this subject either as to the nature of the disease or its prevention. This latter will, without doubt, form the most prominent surgical question in any future war between civilised nations. It may with truth be said that up to the present time the efforts of surgeons in this direction during war have not been successful, but since the Franco-German war great advances have been made in the use of antiseptics, both rigidly as by Lister's method, or more laxly as employed by many British and foreign surgeons. To any one who has seen a military hospital about ten days after a battle, the occurrence of pyæmia will not be a matter of surprise. A gunshot wound of the chest if the patient survive for any length of time may often suffice to poison the air for many yards round. The offensive discharge is often so abundant as to soak through the patient's bedclothes and even his mattress more than once in the day. In a civil hospital such a case would in all probability be removed to a private ward, but in the temporary hospitals during war this is usually impossible. No civil surgeon would feel quite easy if from any cause he found his ward containing an average of more than one half of severe open wounds, and yet in military surgery every patient is suffering from an open wound, always suppurating and often sloughing. In all probability the best dressings will be found to be carbolic acid and oil next the skin, covered by large pads of oakum. It is an interesting fact that in spite of the unfavorable conditions in which the wounded were placed, and the comparative frequency of pyæmia, true cutaneous erysipelas was observed by Dr. Fieber in only one case. This fact, which is by no means an unusual observation, seems to support somewhat the view that pyæmia and erysipelas are in no way connected with each other, and that for the production of erysipelas a specific poison is required. Hospital gangrene was much more abundant and was invariably treated in tents, and with such success that of forty-three cases only seven died. The local treatment consisted in the application of fuming nitric acid.

The author devotes some pages to the treatment of secondary hæmorrhage, and we regret to see that what we cannot but consider a retrograde step in surgery is by no means unpopular amongst German surgeons. We had thought that the labours of Guthrie and other British surgeons during the earlier part of the present century had quite settled the rule of practice to ligature for primary or secondary hæmorrhage at the bleeding spot whenever possible. No doubt the operation is often difficult and sometimes impossible from the condition or situation of the wound. Dr.

Fieber, however, considers the proper line of practice for secondary hæmorrhage is to tie the main artery in its continuity and not a short way above the bleeding point, as has sometimes been recommended, but high up so as to offer as great an impediment to the return of blood as possible. Thus, for secondary hæmorrhage from a thigh stump he would ligature the external iliac and for that from a wound of the forearm he would tie the brachial above the superior profunda. He would proceed to do this at once without attempting even to find the bleeding point. In support of these views he quotes Bernhard, Beck, and some others. From his not quoting the works of any English author or the reports of the Army Medical Department of the United States we suppose that he had no access to the best arguments on the other side. We can readily understand that in gunshot wounds there may be cases in which, when hæmorrhage sets in on the separation of the sloughs, a surgeon may hesitate to go in search of the bleeding point possibly situated deeply in the swollen and infiltrated tissues, but how any surgeon can extend such a principle to secondary hæmorrhage from stumps passes our comprehension. Secondary hæmorrhage very rarely takes place from a wound, that is to any extent united. In the vast majority of cases it occurs from sloughy unhealthy wounds which are all the better for being fully opened up and cleaned out. Moreover, it must have occurred to every surgeon of any experience to open up a wound in the expectation of finding the hæmorrhage proceeding from a large vessel, and on the contrary to discover no recognisable vessel, or at most only a few small points which cease bleeding as soon as they are exposed to the air. It would be a terrible blunder to expose the patient to the danger of ligature of one of the main arteries for such a condition as this. Dr. Fieber lays great stress upon the danger of making a fresh incision in the midst of unhealthy suppurating parts by which the absorption of septic matter might readily occur, but this danger may be easily overcome by wiping the foul surface over with a strong solution of chloride of zinc. In short, we see nothing in his arguments to justify us in abandoning the well-established rule of ligaturing, whenever possible, at the bleeding point. The rest of the pamphlet is devoted to various wounds, but although containing many interesting cases presents nothing of sufficient importance to require notice.

Analysis of Six Hundred and Seventeen Cases of Skin Disease.¹—Dr. Bulkley has here presented to the medical world the result of a year's experience in diseases of the skin at a large public dispensary, the number of cases analysed being 617. The cases are

¹ *Analysis of Six Hundred and Seventeen Cases of Skin Disease, with Cases and Remarks on Treatment.* By L. DUNCAN BULKLEY, A.M., M.D. Pp. 41. New York, 1876.

arranged in two tables, the first giving an alphabetical list of the diseases and the number of instances of each; and the second arranged in the order of their frequency, showing also the varieties of each disease, and with a column of their percentage occurrence. Eczema stands first in the list in regard to frequency, forming about 35 per cent. of all the cases, but this designation includes impetigo and many forms of lichen. The syphilitic diseases, which Dr. Bulkeley includes under the general name of syphilodermata constitute 6·3 per cent. of all the cases, instead of 9·8 per cent. in the previous year. Although there is nothing either very new or very striking in Dr. Bulkeley's little work, he has done good service in publishing it, and we agree with him that those who occupy public positions owe to the profession the record of the experience gained in them, nor can we blame him if some of the maladies described are of comparatively little interest, for, as he says, while rare diseases and new therapeutic measures are often made the subjects of study and report, the more homely details of everyday conflict with diseases of the skin are seldom offered to the medical public.

Experimental and Clinical Researches on the Thorax in Pleurisy, and on Pleurotomy.¹—The object of this essay is to explain the operation in which the pleural cavity is largely opened by means of a cutting instrument, and thus pleurotomy is a different operation from pleurocentesis, or, as it is more generally though perhaps incorrectly termed, thoracentesis. The latter operation is followed only by the replacement in their normal state, as far as possible, of the intra-thoracic organs, while pleurotomy, on the other hand, induces in the organism phenomena altogether novel and very different from the physiological conditions. In cases of intra-pleural effusion Dr. Peyrot considers that pleurotomy is indicated where pleurocentesis is insufficient; and he explains his meaning to be that while the simple puncture of the pleural wall is sufficient in serous effusions, it becomes inadequate whenever the liquid contained in the pleura is in the least degree in a state of putrid fermentation, in which case all the symptoms are aggravated by the presence of pus and putrid gases in the pleural sac.

Pleurotomy has a double object, namely, on the one hand, to afford an easy mode of exit to all the solid, semi-solid, liquid, or gaseous matters contained in the pleural cavity, and, on the other, to establish a permanent and easy communication between this cavity and the external air. The incision which accomplishes these results must be large, and may be performed without danger if attention be paid to the rules laid down by Dr. Peyrot. The fact of the

¹ *Étude expérimentale et clinique sur le Thorax des Pleurétiques et sur la Pleurotomie.* Par le Docteur J. J. PEYROT.

(*Experimental and Clinical Researches on the Thorax in Pleurisy and on Pleurotomy.*) Pp. 153. Paris, 1876.

effusion having being placed beyond a doubt by an exploratory puncture, an incision is to be made first from before backwards and then from behind forwards. But the operation in itself is useless without due attention to the after-treatment, which consists chiefly in maintaining a communication with the external air, allowing a free escape for all the morbid secretions, and washing the pleura frequently and abundantly. This is done by means of two tubes, the liquid entering by one and passing out by the other.

Transactions of Philadelphia College of Physicians.¹—In the January (1876) number of this review we took occasion to introduce to our readers' notice the first volume of the new series of 'Transactions of the Philadelphia College of Physicians,' and to remark on the number of distinguished names to be found in the list of fellows of that college. We have now before us the second volume, which, besides memoirs of two recently deceased eminent fellows, Dr. G. W. Norris and Dr. J. S. Parry, contains thirteen papers of varying length, two or three, indeed, being little else than brief memoranda. The papers generally are of a clinical character, recording cases and the results of treatment. Dr. James H. Hutchinson gives a case of empyema successfully treated by the aspirator and a drainage tube, and Dr. Starr records one of hepatic abscess punctured through the abdominal wall and ending in recovery.

Dr. Cieeman presents a report of the committee on meteorology and epidemics. Excision of the knee is the subject of a paper by Dr. John Ashhurst, jun., illustrated by notes of ten cases, all of them, except one, under fourteen years of age. This paper is supplemented by notes of two cases of excision of the knee in adults by Dr. H. Lenox Hodge. The two contributions together form a valuable addition to the literature of excision. The communication on the therapeutic uses of compressed and rarefied air would, by reason of its subject matter, be attractive at this time, when the value of climate, especially in consumption, is so much debated; but it is too brief and general in its remarks to aid in arriving at a definite opinion. Hysterical affections of the eye are intelligently examined by Dr. Harlan, and the cases related by Drs. Forbes, Mears, Morris, and Tyson, will be read with interest on account of their rarity or peculiarities. But there remains for notice the more considerable and important paper by Dr. Weir Mitchell, entitled "Cases illustrating local injuries of nerves and their trophic consequences, with comments," which, as well for its intrinsic merits as on account of the reputation of its writer, will give a lasting value to this volume of transactions.

¹ *Transactions of the College of Physicians of Philadelphia.* Third Series, vol. ii. Philadelphia, 1876.

The cases are narrated chiefly in illustration of the doctrine of the existence of trophic nerves. They are eight in all. A careful examination of the nervous phenomena appears to have been made, and woodcuts are introduced to exhibit the portions of surface in which sensation was abnormal. A coloured lithograph is also given in illustration of the trophic changes observable in the condition of the hand in every case.

These 'Transactions' should have a place in all our public medical libraries. They will be valued, principally, as a repertory of interesting cases.

Transactions of the American Ophthalmological Society.¹—We have again the pleasure of recording the publication of this useful annual report, though we are sorry to find that it does not contain a preface, an account of the formation of the society, and a notice of the first meeting, as promised. (See this Review, vol. lvi, p. 175, 1875.)

Dr. Webster reports a case of spontaneous cure of subretinal effusion, and adds an abstract of twenty-one cases of detachment of the retina, taken from the case-books of Dr. C. R. Agnew, of New York. The report is to the following effect :—A lady, æt. 57, very myopic, but who had never used glasses, was first seen on June 26th, 1875. Two months previously the vision had become suddenly much worse. She saw only the lower half of an object with the right eye. O. D. gl. oo, $V = \frac{1}{200}$. O. S. gl. oo, $V = \frac{1}{200}$: gl. — $3\frac{1}{2}$, $V = \frac{2}{200}$. There was a prominent separation of lower half of right retina, and a diffuse opacity of the left vitreous. She was treated by rest in bed, not being permitted to quit the recumbent position for a moment for any purpose, and atropia was also used locally. On July 19th no separation could be seen ; the opacity of the vitreous was less. "In both eyes there was extensive staphyloma posticum, with large patches of choroidal atrophy and exudation. O. D. gl. — $1\frac{1}{2}$, $V = \frac{2}{200}$: o. s. gl. — 2, $V = \frac{2}{200}$.

Two of Dr. Agnew's patients were treated unsuccessfully by the recumbent posture. One man, a farmer, in whose eye the detachment had existed for six months, kept his bed for six weeks without improvement ; another, a student, lay on his back four weeks, and rose with vision unimproved.

Dr. Wadsworth mentioned a case in which a small separation just below the macula disappeared without special treatment.

Dr. Strawbridge reported a case of operation :

"The patient was a girl of sixteen. The left eye, which was highly myopic, had been blind for a year from total separation of the

¹ *Transactions of the American Ophthalmological Society. Eleventh Annual Meeting.* Newport, July, 1875. New York, 1876.

retina. The retina of the right eye, ten days before my examination, had commenced to separate in the upper temporal quarter, as could most distinctly be seen with the ophthalmoscope. There was entire failure of the corresponding portion of the visual field. With this eye she could read Jäger No. 11 with great difficulty. I made the Bowman operation for her relief. With two needles, introduced through the sclera, in its upper temporal quarter, a free incision or tear was made in the retina. This was followed immediately by a large subconjunctival chemosis. The next day showed the retina entirely replaced and visual field perfect. Vision was greatly improved. At the present time, five weeks after the operation, patient can read Jäger No. 2, and there is no return of the trouble."

Dr. Mathewson had seen a case of separation of the retina, resulting in recovery, and Dr. Kipp one where confinement to bed for three weeks seemed to seriously damage the general health without causing the least improvement in the eyes.

Dr. E. Williams gives an account of two cases of exophthalmic goitre; in one the operation of tarsoraphy in each eye was followed by sloughing of both corneæ and death; in the other there were some threatening symptoms on the second day after the operation. Dr. Williams believes—

"That the pressure of the narrowed commissure, in both these cases, caused strangulation and the consequent inflammation. In the fatal case the nutritive powers were at a low ebb, and the slightly increased pressure strangulated the corneæ. In the other case the conjunctival and corneal trouble were concentrated at the outer margins corresponding to the rigid commissures. If I make this operation again I shall pare and close the lids in the centre as well, so as to be sure of keeping the eyes closed, distributing the pressure and guarding against danger to the cornea. After a few days, when the eye is accustomed to the new conditions, the central adhesion might be detached with safety."

Dr. G. Strawbridge describes and gives a drawing of a growth which projected from the region of the disc so far forwards that it could be seen by oblique illumination. It was apparently composed of connective tissue, and contained numerous vessels.

Dr. G. C. Harlan continues the history of two cases of vascular disease in the orbit, which had been under his care about six years ago. He considers the disease in the one case to be an aneurism by anastomosis in the orbit. He says, as to the second, that when it

"was reported before, I had not the slightest hesitation in calling it an 'orbital aneurism;' but the more I have looked into the subject the less confident I have been of my opinion, and in view of the admitted difficulties of diagnosis and the many recorded mistakes of

the high authorities, I have thought it safer to retire behind the comprehensive term of 'pulsating exophthalmus.' "

The author agrees with Nunneley's opinion, that in the great majority of cases which have been attributed to orbital aneurism there is no disease whatever in the orbit, but that the symptoms depend on obstruction to the return of blood through the ophthalmic vein. An analysis of fourteen post-mortem examinations, which are all that the author can find, gives—

"Venous clot from phlebitis, four cases.

"Venous dilatation from phlebitis, two cases.

"Malignant growths, three cases.

"Post-orbital aneurism, three cases.

"Post-orbital varix, two cases.

"Competent observers had committed themselves to the diagnosis of orbital aneurism in nearly all these cases, and in the remainder the same conclusion could hardly have been resisted by any surgeon whose attention had not been particularly called to the subject. In only five cases was there anything like an aneurism, and in them the arterial lesion was so situated that it could be the cause of the orbital symptoms only in the same manner as any other obstruction to the venous flow might be."

Other cases and papers are—Dr. Pomeroy : a case of nystagmus associated with strabismus, relieved by tenotomy. Dr. E. Williams : cases of displacement of the lens, of anæsthesia retinæ, of binocular temporal hemiopia, and of strabismus with rigidity of the muscle. Dr. Strawbridge : hysterical blepharospasm, relieved by forcible elevation of the eyelid, cases of foreign bodies in the eye, and a case in which there were crystals of cholesterine in the anterior chamber and on the anterior surface of the lens, as well as in the vitreous. Dr. G. T. Stevens : successful extraction of a foreign body from the retina by the aid of the ophthalmoscope. Dr. W. Thompson : paper on the choroidal cone, which we have already noticed, and a case of a sector-like defect in the field of vision. Dr. G. Hay, on the changes in the refraction of a plano-cylindrical lens when rotated about its axis. Dr. O. F. Wadsworth : on the effect of a cylindrical lens with vertical axis placed before one eye. Drs. McKay and Eno : a case of general sarcoma of the choroid. Drs. Agnew and Eno : a case of enucleation of both eyes for glioma (this and the previous case are illustrated by some charming etchings by Dr. Eno). Dr. J. Green : on iridotomy, examination of the eyes of a criminal executed by hanging, and on castor oil as a menstruum for dissolving atropia. Dr. Noyes : improvements in spectacle frames, and a case of retinal separation in which the fluid was spontaneously absorbed.—T. W.

On Post-mortem Examinations and Method of Performing them.¹

—This pamphlet consists in a translation, reprinted from the ‘Medical Times and Gazette,’ of a descriptive and explanatory report on the method of performing *post-mortem* examinations in the dead-house of the Berlin Charité Hospital. The work, as will be readily assumed from the reputation of the author, is one of much interest, and constitutes, we believe, an important contribution to the professional literature of the year. It has been well translated, is full of information, and manifests throughout the thoroughness and earnest endeavour by which most German scientific inquiry is characterised. Written with special reference to medico-legal investigation, it may be regarded as a welcome stimulus to improvement in the same kind of research in this country. It may, moreover, be perused with advantage by our teachers as well as by our students of pathological anatomy. We find in the pamphlet a recognition of the fact that macroscopic investigation of the organs of the human body is quite as important as the examination under the lens of “sections and scrapings.” The great master of modern pathology deals not here with cells, nuclei, and stroma, but discusses the coarser work of the dead-house, describing the kind of knife to use, and the manner in which to make with it free and proper incisions. Professor Virchow insists much on the importance of a definite order of sequence in every autopsy and in every detail of the examination. He refuses to regard pathological anatomy as merely a supplement to clinical medicine. The course of the anatomical investigation should not be decided, it is held, by the clinical history of the case, since the advance of medical science can be furthered in a more essential manner by the acquisition of the habit of submitting not merely the organ supposed to be diseased, but all the other organs of the body to a minute examination. Pathological anatomy is regarded as equally important in relation to forensic medicine; the former, as it is more universal, and has to deal with cases of all kinds, serves to protect against the results of the limitation of view that is favoured by medico-legal practice. The truth of the following passage will doubtless be fully appreciated by English readers:—

“It must be conceded that the great majority of medico-legal reports have exhibited such an astonishing sameness, even in their phraseology; such a very peculiar style nowhere else to be found; such a want of real objectivity, that it is an extremely tedious business to read through any number of them consecutively.

¹ *On Post-mortem Examinations and the Method of performing them.* By Professor RUDOLPH VIRCHOW, of Berlin.

There was such an amount of similarity among many of them that it might have been thought that they all referred to the same case."

As an important detail of his definite process and prescribed order of *post-mortem* examination, Professor Virchow asserts that the abdomen should be opened before the thorax in order that the position of the diaphragm may be ascertained. This cavity having been opened, the operator does not at once proceed to dissect and remove the abdominal viscera, but, after having determined the position of the diaphragm and of the organs below it, the colour and form of the exposed parts, and the presence or absence of any abnormal contents, commences with his examination of the thoracic cavity. It is pointed out as a reason for such direction, that the abdomen should be opened before the chest, but that the heart and lungs should be dissected and removed before the abdominal viscera, that the position of the diaphragm and abdominal viscera cannot be exactly determined after the thorax has been opened, and, on the other hand, that division of the inferior vena cava and of the hepatic veins in the removal of the liver must result in a partial or even a complete escape of blood from the right auricle.

After some interesting remarks on the colour of the blood in the cadaver, and on what the author regards as the "much misused" term of vascular fulness, we meet with minute directions as to the best mode of examining the viscera, special attention being given to the heart and brain. The pamphlet concludes with some reports of autopsies, drawn up according to the regulations of the Imperial Committee of Science for Medical Affairs. These reports, though they may be regarded as too elaborate for strictly pathological purposes, may be recommended as models of medico-legal cases, and cannot, we think, be too closely followed in the preparation of such records of medical testimony and observation.

Atlas of Topographical Anatomy.¹—Mr. Bellamy and his publishers, Messrs. Churchill, deserve much credit for bringing out this volume of plates with accompanying letter-press in elucidation of them. The undertaking must be attended with much cost, and it deserves the support of the profession. It is the first attempt in this country at a complete atlas of topographical anatomy, wherein sections of the trunk and limbs are presented so as to show the relative size and position of the several parts and structures, as they exist naturally and undisturbed by the knife of the anatomist.

¹ *An Atlas of Topographical Anatomy after Plane Sections of Frozen Bodies.* By W. BRAUNE. With 46 woodcuts in the text. Translated by EDWARD BELLAMY, F.R.C.S. London, 1877.

Figures, indeed, of similar character, are not wanting scattered in works on descriptive anatomy, on practical surgery, and on surgical anatomy; but we have in this volume a consecutive series, by which an accurate conception of the relations of tissues and organs may be reached, such as is elsewhere unattainable. Their value will be at once appreciated by the physician, but to a greater degree probably by the surgeon.

The edition of which Mr. Bellamy has made the translation, is a reproduction of plates representing plane sections of the human body originally drawn life-size, but now reduced to a smaller scale. The name and reputation of the author, Wilhelm Braune, professor of anatomy in the University of Leipzig, affords a guarantee of correctness both of figures and of descriptions, as well as of the practical value of many accompanying remarks indicating the lessons to be gained from them both to surgeons and physicians.

In not a few, indeed, and especially among the woodcuts printed in the text, the physician can learn much of the effects of disease in displacing organs, and thereby gain instruction in making his diagnosis, and in reading the signs of disease. Professor Braune writes, in his preface, "The necessity of such plates for the clinic has repeatedly been expressed, and especially for military surgery. It has been emphatically stated on all sides that the sections of the thorax and abdomen are indispensable for diagnosis, and that the examination of my plates has been of substantial assistance in judging correctly the direction of a gunshot wound."

Where the purpose and utility of a work are so self-evident, and its performance so satisfactory, the notice of a reviewer may be of value in introducing it to the public, but its own merits will recommend it more than the praises the critic has to bestow.

German Clinical Lectures.¹—The publication by the Sydenham Society of a volume of clinical lectures on various subjects connected with the several principal branches of medical practice we consider to be a happy thought. At the present day we gain much insight into the modes of practice pursued on the Continent by means of retrospects and of systematic works translated into English; but a volume of lectures such as the one before us more fully puts us in possession of the views of foreign physicians concerning both pathology and treatment.

In a clinical lecture the physician will often take opportunity to open out a new hypothesis, and to descant on the why and wherefore of his opinions and practice more at large and more intelligibly than in any systematic work he may write, in which condensation of matter must be a paramount consideration, and a logical handling

¹ *Clinical Lectures on Subjects connected with Medicine, Surgery, and Obstetrics by various German Authors.* Published by the New Sydenham Society, 1876.

appear essential, even if it be at some sacrifice of clearness and ready apprehension. In short, we have greater faith in the instructive qualities of clinical lectures than of chapters in systematic treatises much more ambitious of completeness in matter and composition. We believe that clinical lectures especially recommend themselves to medical men in practice; whilst to students, in anticipation of examination, who have to undergo cramming, the systematic treatises are probably more useful.

It is time, however, to acquaint our readers with the contents of this volume. They are, as the title indicates, miscellaneous in character; and they are, what is important, the production of men eminent in their profession as pathologists and practitioners. Some of the subjects chosen will attract by reason of their recondite pathology, whilst others will secure attention by their value as therapeutical essays. The first lecture is by Professor Kussmaul, of Berlin, and is on bulbar paralysis and its relationship to progressive muscular atrophy; the second, by Professor Ziemssen, details his views respecting the treatment of simple ulcer of the stomach; the third is by Professor Eschscholtz, than whom no one is better qualified to deal with its subject, which is "the artificial emptying of blood-vessels in operations;" the fourth, on lupus and its treatment, is by the illustrious surgeon Volkmann, who is likewise the contributor of two other lectures, one on infantile paralysis and paralytic contractions, the other on excision of joints. Reflex paralysis is discoursed on by Professor Leyden, to whom we are indebted for much knowledge of the nervous system; Biermer, of Zurich, treats of bronchial asthma; Bartels, of Kiel, of the various forms of chronic diffuse nephritis; Professor Hildebrandt, of catarrh of the female genital organs; Liebermeister, who has made fevers and contagion a special study, of the treatment of the pyrexial state; Professor Jürgensen, of the treatment of croupous pneumonia; and Professor Ruhle, of Bonn, of diseases of the pharynx; whilst obstetricians will be glad to read the lecture of Professor Olshausen, of Halle, on parametritis and perimetritis.

We have enumerated the titles of fifteen lectures, any one of which will be read with advantage and instruction, and we feel convinced that the volume will be gratefully received by the members of the Sydenham Society, and that all who become acquainted with it will learn to admire the thoroughness of German teaching in pathology, and the excellency of German practice, guided as this is, so far as the state of knowledge permits, by accurate observation of the effects of remedies, and by consideration of the producing causes of lesions and their modifying influences on healthy tissues and functions.

Dobell's Reports on Chest Diseases.¹—We are glad to find Dr. Dobell to have received sufficient encouragement to persevere in the very considerable undertaking of producing an annual volume of 'Reports on Chest Diseases.' As a work consisting of extracts, condensed or otherwise, from the periodicals, the pamphlets, and the published treatises of most of the civilised nations of the earth, its value is for reference to those who are interested in any special matter connected with thoracic affections. The attempt to read it through would, indeed, be a task few would undertake, and even should it be accomplished the reader would rise from it rather confused than enlightened. In fact, as a collection of facts, opinions, and hypotheses or guesses, gathered on all sides by diligent collectors, it necessarily records facts of varying value as data, and hypotheses or opinions of doubtful value, and, at times, jarring and irreconcilable one with another. Nevertheless, the volume is a painstaking production, and can take rank with the best abstracts or retrospects published.

Study of the Semeiological Value of Ecthyma.²—Commencing with the observation that the study of diseases of the skin leads to the conviction that these maladies have no individual existence, but are only the manifestations of various morbid conditions, Dr. Muselier proceeds to examine in detail the various modes in which ecthyma develops itself and the different diseases which it accompanies or follows. His essay is divided into three chapters, in which he treats successively of the Semeiotic value of Acute Ecthyma, of the chronic form, and of the disease generally in relation to Syphilis. His general opinion is, that ecthyma may be developed in various forms of disease and may indeed appear in affections so different from one another as smallpox and itch. His general conclusions on the subject are expressed by himself in the following terms. Ecthyma is seldom an idiopathic disease, and is always secondary, generally following some affection due to local irritation, such as occurs in the cases known as parasitic. Chronic ecthyma is a frequent complication in all cases where cachexia predominates, as in chronic diseases, in serious cases of fever, in debility resulting from unfavorable hygienic conditions; and it may also appear as a critical eruption, or in the course of certain affections of the nervous system. It is one of the

¹ *Annual Reports on Diseases of the Chest.* Under the direction of HORACE DOBELL, M.D., assisted by numerous and distinguished coadjutors in different parts of the world. Vol. ii, June 1st, 1875, to June 1st, 1876. London, 1876.

² *Étude sur la Valeur Séméiologique de l'Ecthyma.* (Accompagnée d'observations recueillies à l'Hôpital Saint-Louis.) Rapports de l'Ecthyma avec la Syphilis. Par PAUL MUSELIER, Docteur en Médecine.

(*A Study of the Semeiological Value of Ecthyma. Accompanied by Cases observed at the Hôpital St.-Louis. Relations of Ecthyma with Syphilis.*) Pp. 126. Paris, 1876.

most serious cutaneous manifestations of syphilis, and its development and progress in this disease is remarkably influenced by the intervention of cachectic conditions.

Roberts's Handbook of Medicine.¹—The appearance of a new edition following so speedily upon the previous one, especially when the work is of so considerable dimensions as the one before us, is at least a sufficient indication that it meets the requirements of a large number of readers. The treatise is, in fact, a student's handbook, and we are not surprised at its success, for it contrives to convey in a comparatively moderate space an excellent and very complete survey of the principles and practice of medicine, as accepted by the leading authorities in physic of the present day. It is a godsend to students to have within their reach a volume of this character, which conveys just the amount of knowledge likely to be required of them at examinations, conveyed in a dogmatic manner, so as to save them the perplexity of seeking conclusions for themselves out of a multitude of theories in pathology and practice which some more comprehensive works of medicine place before them.

Students will also better like the treatise because it follows in the arrangement of its subjects the order sanctioned by long usage, and attempts no novelties in that direction, for novelties in such matters are only likely to embarrass when examination day arrives.

Such being the character of the work, it does not claim an extended notice from a reviewer; it is sufficient to recommend it as an excellent digest of the principles and practice of medicine as accepted at the present time, and to add that it is put forth by the publisher in a very readable and very creditable form, a meed of praise which cannot be accorded to all handbooks of medicine.

Phillippo on Climate of Jamaica.²—The climate of Jamaica is, as the title-page would indicate, the subject-matter of this little volume of eighty-two pages; but, as the list of "contents" shows, it is really only one of several matters dealt with. For instance, we are presented with three chapters or sections, viz. "(1) on the voyage to Jamaica, (2) Kingston and the Lowlands, (3) from Kingston to the mountains," occupying together thirty pages of the book, before we reach the chapter on the climate of the island, which occupies only twenty pages. And when we proceed to examine this chapter we find no little irrelevant matter, scarcely

¹ *A Handbook of the Theory and Practice of Medicine.* By FREDERICK T. ROBERTS, M.D. Second edition. London, 1876.

² *The Climate of Jamaica.* By JAMES CECIL PHILLIPPO, M.D. London, 1876.

anything derived from the author's own observations respecting the climate, and only an indifferent selection of quotations from a few authors who have written descriptions of the island or have collated and commented on the medical statistics respecting the health and mortality of the troops stationed in some localities. The three remaining chapters are occupied severally with remarks on "tubercular disease," "on the diseases of Jamaica," and on "the mineral springs of Jamaica," but in neither of these will the reader find special and valuable information such as he might naturally look for in a special treatise. In fine, as a real and useful contribution to our knowledge of the medical and hygienic features of Jamaica, this book must be pronounced a failure.

Beasley's Book of Prescriptions.¹—That a fifth edition of this book of prescriptions is called for argues that, in the eyes of no small number of people within and without the ranks of the profession, it possesses a real value and utility. For our part we must confess to a want of appreciation of this and similar collections of prescriptions. A vast number will be found of a very commonplace character; others difficult to dispense, complicated and fanciful; others empirical and based upon peculiar if not also erroneous pathological notions. Indeed, the study of formulæ recommended for this or that particular disease is an encouragement to empiricism, for the whole value of ready cut and dried prescriptions rests upon the assumption that disease is a fixed and unvarying entity, and that we may treat it by means of a certain combination of drugs. We are ready to admit that experience has shown that certain combinations are peculiarly valuable, and certain others to be avoided; but this is a lesson to be learnt from therapeutical treatises, and so learnt that the well-informed practitioner can turn it to account for himself as required, keeping the combination in mind, just as he does the properties of the usual pharmaceutical preparations. It would argue a want of knowledge of the *materia medica* for a medical man to be unable to concoct his own prescriptions, and a want of due apprehension of the character of diseased action as affected by individualities to resort to a particular formula, recommended probably by some individual of whom he may know nothing, as a suitable remedy for a morbid condition to which is affixed a special name.

But though we urge these general objections to collections of prescriptions we are not prepared to condemn them altogether or to deny to them a certain utility, if prudently and with judgment re-

¹ *The Book of Prescriptions, containing more than 3000 prescriptions collected from the practice of the most eminent physicians and surgeons, English and Foreign, &c.* By HENRY BEASLEY. Fifth edition. London, 1876.

ferred to by intelligent practitioners. If rightly used there are many recorded prescriptions of considerable value, which may in some forms of disease be copied literally, and there are many more which, if not adopted *in toto*, are at least suggestive. At the same time we must repeat our conviction that their use favours empiricism, and will further record our impression that books of prescriptions find special favour with prescribing chemists and with self-doctoring patients, and that therefore they are accountable for a certain amount of mischief and quackery.

Original Communications.

I.—Notes on Syphilis in the Insane.—By W. Julius Mickle, M.D.,
Medical Superintendent, Grove Hall Asylum, London.

PART III.¹

The Distinctions between Syphilitic Disease of the Encephalon and General Paralysis.

AT the outset I may state that a study of numerous cases of general paralysis of the insane, with complete histories of the life-symptoms and post-mortem appearances, and of a number of cases in which constitutional syphilis existed, has not confirmed the view of some Continental observers that syphilis is the cause, or one of the causes, of general paralysis. In speaking of the clinical similarities and of the diagnosis between brain syphilis and general paralysis it must, therefore, be premised that these affections are distinct and independent. The post-mortem appearances alone are sufficiently conclusive in relation to this. In general paralysis are those widely spread changes in the brain, spinal cord, and meninges which need not be detailed here, but which, though they blend with the morbid anatomy of syphilis at points, are, on the whole, distinct from the gummatous infiltrations, nodules or tumours, the arterial changes, the pachymeningitis, the localised meningitis, the ostitis, caries, and periostitis, of syphilis. It is true that in brain syphilis widespread alterations may be found in the cortical and central grey matter, in the conducting medullary fibres, and, above all, a widespread alteration in the minute cerebral and meningeal blood-vessels, which may cause a deterioration or derangement—intellectual, moral, and emotional—as great as that found in general paralysis. But well-marked differences obtain even here in the microscopical anatomy, as may be seen by comparing such of the minute changes in general paralysis as are well authenticated with the microscopical appearances detailed in a former paper by the present writer (this Review, July, 1876, pages 168 to 173), and found in those cases

¹ Parts I and II, published in 'British and Foreign Medico-Chirurgical Review,' July and October, 1876.

of syphilis which simulate the features of general paralysis, more or less.

The *clinical* distinctions are, however, of much greater *practical* importance. The two affections may coexist, and, indeed, many of those who become general paralytics have been the very individuals likely to expose themselves recklessly to sources of syphilitic infection.

Still more interesting and important are those cases in which intra-cranial syphilis simulates one or other of the symptomatological forms of general paralysis; cases in which the patients are not necessarily doomed, as those are whose intra-cranial syphilis hurries on the fatal termination of their general paralysis. Not necessarily doomed, but the only hope for them rests in a correct diagnosis, and in a vigorous, judicious, and prolonged specific and general treatment founded thereupon. Thus, the differential diagnosis becomes of the greatest importance in cases of this kind. Intra-cranial syphilis may produce symptoms more or less closely resembling general paralysis. The form¹ most frequently assumed is (1) that of general paralysis in which the mental symptoms throughout, or very prominently, are those of psychical failure and weakness, running a more or less unchecked and regular course to fatuity. The next in order of frequency of its simulation by brain syphilis is (2) general paralysis of the more striking form, that in which *bien être*, exaltation, or mania of grandeurs is present, during some periods at least. In both these instances certain motor and sensory symptoms must occur in the syphilitic cases ere the resemblance to general paralysis is established.

1. When marked mental symptoms arise from the lesions of intra-cranial syphilis they, for the most part, constitute a dementia. Sooner or later is this the case in those who become lunatic-asylum patients. Outbreaks of mania may occur early, or may complicate the disintegration of mind, and the first mental symptom of all may be, it is said, a hypochondriasis or a melancholy; but withal there is ever a dementia in the background, a slow crumbling away of the mental powers, or a fitful destruction of them by the cerebral accidents incident to the syphilitic invasion. Premising that I refer to the experience of lunatic hospitals, and therefore to cases in which the mental symptoms (in so far as a negation of mind is a symptom) are unusually well marked, the summary given in part 2 of the more usual mental symptoms which accompany intra-cranial syphilis may be reproduced here. "The first (clinical feature) is the marked tendency to mental impairment, failure, or dilapidation; the predominance of negative intellectual symptoms, often associated with

¹ Several forms of general paralysis corresponding to certain differences in the symptoms are spoken of throughout this paper, but merely as a convenience in clinical description.

failure, weakness, or loss of the moral sense and the general inclination to a degraded state of feeling, as evidenced by the habits of the patients. In some, early or intercurrent outbreaks of maniacal symptoms occurred; in a few there was causeless depression or fear, or emotional weakness."

Now, this is a description which in its general outlines applies also to the mental phenomena of many a case of general paralysis. Altogether, too much has been made of the exalted delusions and the expansive feeling of some general paralytics. These brilliant coruscations, signalling a destructive conflagration of mind, are, indeed, striking phenomena, and add a dramatic element to the spectacle when they occur. But they are by no means so very frequently present, and in some cases they have but a slight development or a transitory existence, may have entirely passed away ere the patient comes under care, and, unnoticed or forgotten, may find no place in the history furnished to the physician. In many instances they are entirely absent throughout the whole course of the affection; intellectual decay sets in early and progresses to fatuity, chequered only by intercurrent conditions of confusion and sub-excitement, in which the patient is restless, fidgety, destructive, denudes or defiles himself by day or night, or wanders in an aimless way, and mulishly resists everything that is done for, or with, him; and yet the physical symptoms, the general course and duration of the disease, and the post-mortem appearances, are fundamentally the same as we find when the typical grandiose delirium has been present. Fundamentally the same, but differing in slight details, and not differing more from those of the typical paralytic mania of grandeur, as described, than do the cases of this amongst themselves.

This *dementia* form of general paralysis is sometimes simulated by brain syphilis, and not only as to the mental symptoms, but in part as to the physical also; its physical symptoms, indeed, are often more akin to those of brain syphilis than are those of the other forms of general paralysis; and it is better for our present purpose to examine the two diseases and contrast them as far as possible by choosing the varieties of case in which their clinical similarity is greatest. It is greatest where there is a distinct history of constitutional syphilis, or evidences of past or present syphilis, in certain demented general paralytics. This history or evidence of constitutional syphilis are often much more readily obtained or obvious in general paralysis-dementia than in the cases of some actual subjects of intra-cranial syphilis. In distinguishing between cases such as these a study of the symptoms in all their relations alone can guide us, aided sometimes by the diagnostic significance of the effects of specific treatment.

Using cases of general paralysis in which dementia obtains from the first, and some of which have the history or traces of constitu-

tional syphilis, as being those in which the distinction from encephalic syphilis is the more difficult, some differences are found in the mode of onset and early symptoms of the two affections. Of the earlier symptoms of general paralysis with predominant dementia, and sometimes complicated with a syphilitic history, the following summaries may be taken as illustrations. All the patients were soldiers.

1. Aged 27. Mental disease insidious in origin, but had been marked for several months before admission. Cause unknown. There had been constitutional syphilis, at what date not stated. There was gradual mental failure, confusion of thought, defective memory, insomnia, restlessness, occasional terror and excitement, propensity to wander about in an aimless and listless way, or to collect and hoard rubbish in his pockets, which upon one occasion he is said to have called "diamonds." There were also temporary right hemiplegia, continued and increasing impairment of articulation, tremors of the muscles of the tongue and lips, especially during speech; later, a tremulous state of the hands, general tremulousness when standing, and slight right hemiplegia, which was paroxysmally worse; also troublesome habits of undressing, of clothes-tearing, and of involuntary passages. The motor symptoms, &c., of general paralysis became fully developed, and no lesion characteristic of brain syphilis was found after death.

2. Aged 39. Of long service; mental disease of three months' duration when admitted, and the assigned cause indefinite; had had constitutional syphilis fifteen years previously. Mental weakness increased rapidly, memory failed, at times he was restless and fidgety; incomplete right hemiplegia came on without any accompanying seizure. Later, fatuity increased, othæmatoma developed; for some time traces of the hemiplegia remained; convulsions and spasms, entirely or mainly unilateral, recurred frequently, and affected sometimes one and sometimes the other side, often occurring as a typical "hemispasm." There was general tremulousness, especially when standing, and once choreiform movements were superimposed upon the constant decided tremulousness of the right upper extremity, while now and then temporary incomplete palsies followed the more severe convulsions. The speech, state of lips and tongue, and universal motor paresis, were characteristic of general paralysis, and the necropsy yielded evidence of only that disease of the nervous system.

3. Age 31; long service; temperate; no constitutional syphilis; had been exposed to the sun in the East four years previously. Mental impairment is gradual and insidious. His temper changes; he becomes irritable, passionate, and at times even violent towards his wife, and from time to time exhibits considerable mental excitement and restlessness, but still continues at his duties. Neglecting a military duty, he is reduced to the ranks, and the alteration in

manner and disposition thenceforward becomes still greater, he grows sullen and taciturn, and suffers much from headache, so severely, indeed, as to necessitate his admission into hospital for a time, some months prior to his final admission for manifest mental disease. His eccentricities become more marked, memory fails, the reasoning and perceptive powers are blunted, and speech becomes thick and hesitating, while tremor coactus, and impaired sensibility of the hands, neck, and trunk, &c., come on, and the usual motor symptoms of general paralysis grow more marked. Finally, the lesions of general paralysis alone are seen in the nervous system at the autopsy.

4. Age 37. Mental symptoms of three months' duration when admitted. No constitutional syphilis; said to have had a sunstroke long before, and a blow upon the head at the siege of Lucknow; had suffered from "hepatitis" in India, but was said to be of temperate habits. A convulsion occurred, and was followed by loss of memory, defective hearing, impaired articulation, and weakness of the lower limbs. Then he became irrational, incoherent, restless, untidy, prone to wander, inattentive to the calls of nature, suffered from insomnia, and there was a doubtful statement as to temporary hallucinations of hearing. Slight fibrillary tremors affected the tongue and the lips, especially during speech, which was thick, broken, and hesitating. Later, the universal motor paresis, &c., increased as well as the dementia; and the post-mortem appearances were those of general paralysis.

In these I have chosen instances of the *dementia* form of general paralysis, not unlike certain cases of brain syphilis in their onset and earlier symptoms. Compare them with the mode of onset and earlier symptoms of the form of mental disorder from brain syphilis which simulates general paralysis most frequently and closely. While there is much that is similar in the syphilitic cases, yet, on the other hand, we may find in them an early hypochondria or temporary acute mania or delirium, and the mental symptoms are often *preceded* by marked motor or sensory disorders. Early paralysis of some of the cranial nerves with ocular troubles, early optic neuritis, or local anæsthesiæ, often characterise the syphilitic cases. Headache, nocturnal, deeply seated, and increased by pressure or warmth, is usually a striking phenomenon, and is more urgent and persistent than is the prodromal headache observed in some cases of general paralysis. Convulsions and local spasms are more frequent in the first periods than in the corresponding periods of general paralysis. Early insomnia, common in some instances to both, is, as a rule, more severe in the syphilitic cases, and the same remark applies to rheumatoid pains in the extremities and to the various neuralgiæ at the same period. Aphasia, too, occasionally occurs at the onset of the syphilitic malady in question, and so do grave apoplecticiform seizures. A fitful appearance, changeableness, temporary

character, and capricious association and succession of the several symptoms and their frequent alternations, are features more evident in the syphilitic cases. Anemia is frequent and a sallow cachectic hue, not usual to general paralytics. The early or rapid decline of memory sometimes insisted upon is, however, common to both; nor is there any very marked difference between the occasional early convulsive phenomena of the two diseases, which in both are commonly unilateral, and often without loss of consciousness, though possibly more often of this type in the syphilitic cases. The impaired articulation and tremors of lips, face, and tongue may be absent or but imperfectly marked; but, on the other hand, they may be imperfectly marked in general paralysis-dementia also. Early well-marked apoplectiform attacks, severe nocturnal cranial pain, paralysis of individual cranial nerves, local muscular contraction and rigidity, optic neuritis and vertigo, failure of special senses, and local anæsthesia, are the most important distinguishing features of (intra-cranial) syphilitic cases in the incipient stages; and these, when present, greatly facilitate the differential diagnosis between the particular class of cases of intra-cranial syphilis now under discussion and general paralysis-dementia. On the other hand, the characteristic condition of the articulation, facial and labial muscles, tongue, velum and pharynx, when fully developed at this early stage, often suffice to establish the diagnosis of general paralysis. It may be added that the age of the patients may often be of service here, inasmuch as general paralysis is rarely noticed under the age of twenty-five, while cerebral syphilis may occur before that age. In soldiers, however, general paralysis occurs at an earlier age (on the average) than in the civil community, and hence the age of the patients has rarely been of service to me in this relations. The ordinary indications of tertiary or the history of constitutional syphilis may, of course, be present in both, and some of the cases already summarised were examples of this. A history of alcoholic or other excesses, cranial injury, or exposure to heat and exhaustion, would assist to a diagnosis of general paralysis as distinguished from encephalic syphilis.

Thus far the mode of onset and the earlier symptoms.

The fully-developed affections may now be described and their features contrasted in detail.

Mental Symptoms.—The following are the most frequent mental symptoms in the *dementia* form of general paralysis:

General intellectual weakness and loss of memory, increasing to fatuity if the patient survives long enough, and evinced by blunting of perception, of apprehension, and of reasoning power, by confusion of thought and of expression, and a slowness and dulness of mental operation, the patient also becoming paroxysmally more "lost."

Incoherence of speech.

Long periods, or paroxysms, in which the patient is restless, meddlesome, prone to self-stripping, destructive to clothing and bedding, or apt to wander about listlessly.

The habits soon become degraded. In some cases occasional paroxysms of excitement arise; in a few there are delusions of impending danger or of injury, together with terror, in the earlier stages, and at this time the patients may attempt to escape from the vaguely apprehended evil; in a few also insomnia, or even hallucination of hearing, is noticed, but this last is very rare.

The emotional display and emotional facility are less marked in this than in any other of the symptomatological forms of general paralysis. Still, at an early period there may be found a slightly expansive state of feeling, a shadowy gaiety of self-consciousness, which, however, soon disappears; or, on the other hand, an irascibility or petulance upon the slightest occasion.

Now, upon comparing *the mental symptoms* of cases of syphilis which simulate general paralysis-dementia with those outlined just above, we find that in the syphilitic cases though the resemblance may be wonderfully close yet a few points of difference may usually be traced. For *in the syphilitic cases*, as compared with the others—

1. Hypochondriacal feeling and idea are more often noticed at the very first, yielding gradually to dementia.

2. Prior to severe apoplectiform, &c., attacks the loss of memory, confusion of thought, and tendency to fatuity are less marked, advanced, and fixed, than in general paralysis-dementia; there is more an obscuration than a destruction of mind.

3. Paroxysmal outbreaks of excitement in action and language are rather more frequent in the syphilitic cases.

4. The course of the mental affection is a more chequered and irregular one, and is more frequently complicated by intercurrent grave nervous symptoms, such as convulsions, spasms, paralyzes, and sensorial disorders, in syphilis.

5. Insomnia and *irritability of temper* are more frequent and marked in the syphilitic cases, but to this there are exceptions on both sides.

Physical Symptoms.—The impairment of speech is characteristic in this as well as in the other forms of general paralysis. At first there is a stumbling, as it were, in articulating certain words, especially those with many consonants, an occasional lingering pause, a hesitation, a quivering and thickness, with indistinctness or elision of one or more syllables here and there, the utterance often being slow and circumspect. Afterwards, articulation becomes more impaired, a more stammering and shaky character is assumed, the words are jumbled together, or there are slowness and stuttering, an aggravation of the former hesitation usually ending in the explosive

utterance of words of which portions are, perhaps, elided; or, with a mumbling and drawling utterance, the voice is thick, hoarse, or coarse. These may finally proceed to the extinction of speech. Voice and utterance are worse after sharp exercise; they may also vary somewhat from period to period, and in the marked cases speech is usually attended by tremulous action of the lips, especially of the upper one, and of the facial muscles, just as in one about to weep. Besides these tremors of the labial and facial muscles during speech, or during speech after sharp exercise, they also, in the advanced cases, may tremble independently of speech or of other voluntary activity of the parts, and the voluntary control over them is gradually lessened. The facial tremor is at times unilateral.

The tongue is affected with fine fibrillary tremor, is often protruded irregularly or imperfectly, or cannot be kept protruded longer than a moment, or quivers and shakes when the patient attempts to use it. Later on the lingual power is still more impaired. The reflex sensibility of the pharynx and soft palate is lessened, and these parts are often relaxed and congested. The laryngeal sensibility and its muscular activity become impaired, and the particles of food tend to pass into the larynx in the later stages.

These affections of speech, of labial and facial muscles, of tongue, velum, pharynx, and larynx, form a group of symptoms which are highly characteristic. When fully developed and coexisting with the usual more general symptoms, they practically clinch the diagnosis of general paralysis, whatever other diseased conditions may be present as complications. But unfortunately their development is by no means complete in all cases, or at least complete at the time when required for practical purposes.

In the syphilitic cases there may be slight tremors of the tongue when it is protruded and a speech deliberate or slightly hesitating, but usually without any accompanying facial or labial tremor; or, again, the voice may be hoarse and low, and there may be hesitation before utterance, which is slow and attended with effort; or a natural hesitation of speech may be aggravated and some mumbling may supervene. But the picture is never complete—the outlines are imperfectly filled in. The tongue more often retains much of its normal condition. The great distinction, however, is that in the syphilitic cases the affection of speech, tongue, &c., when it exists, is almost always mainly of a *paralytic* nature, instead of being of the nature of a mingled weakness and inco-ordination; it is the speech of a cerebral or of a bulbar paralysis, and in the latter case the speech may even become extinct. An early aphasia also is occasionally found in them.

Impaired deglutition not unfrequently occurs in the syphilitic cases as an early and sudden symptom, which may be temporary or

persistent. In general paralysis the impairment of deglutition is gradual and progressive.

In both affections under consideration the features are more or less blank and the lines of expression erased, but in the syphilitic cases the degree in which the physiognomy is blank is less a measure of the impairment of intellect than it is in demented general paralytics.

The ocular conditions also offer points of difference. In the *dementia* form of general paralysis the pupils are in the majority of cases unequal, or unequal at times. In size they may be natural, but often some myosis, much more rarely some mydriasis, is seen in one or both eyes. The pupils are also always more or less sluggish, but the degree of sluggishness not infrequently varies from time to time. Both are usually irregular in outline, or one only is irregular, or the irregularity is intermittent. The optic discs are somewhat atrophied in some cases; in a few white atrophic pallor is evident, and impaired sight or blindness coexists. In the syphilitic cases which simulate general paralysis-dementia there is sometimes irregularity and sluggishness of the pupils; but double optic neuritis, often followed by atrophy of optic discs, or choroiditis disseminata in the earlier periods, are amongst their characteristic features, and hence great impairment of sight or even blindness occur much more frequently than in general paralysis, and are sometimes rather sudden.

Equally characteristic are the palsies of individual cranial nerves, especially of the third, sixth, fifth, or seventh, which often come on suddenly, and, perhaps, disappear after a time, in the syphilitic cases. The syphilitic palsies of these and other cranial nerves are apt to be multifarious, successively developed, recurring, and transitory. They are less frequent where intra-cranial syphilis produces dementia, and thus may be absent in the special class of cases under debate. The ocular troubles produced by these paralyzes are distinguishing phenomena, and are not found in general paralysis except rarely, and then are usually associated with other paralytic lesions owing to *central* mischief, and are in contrast with the usually transient syphilitic implication of the ocular *nerve trunks*. True, an early diplopia may occur in general paralysis, but the slight ptosis and drooping brows of the last stages are merely a part of the general muscular *paresis*, and will not be confounded with the *paralytic* lesions of syphilis. Still, the distinctions are not always made clear by this line of inquiry, and, moreover, the lesions of cranial nerves and the ophthalmoscopic appearances may be absent in this class of syphilitic cases, or when present may arise from some non-specific pathological change; while, on the other hand, in general paralysis the possible effects of hæmorrhage or other meningeal or central complication must not be forgotten.

The Extremities.—In general paralysis-dementia there is gradual impairment of the movements of the limbs, a mingled weakness and inco-ordination. It affects the two sides of the body equally or nearly so. The gait, at first awkward and stiff, becomes at a later period slow, unsafe, swerving, with the feet kept wider apart than usual, and the steps are short, groping, cautious, and shuffling. The patient easily stumbles over an obstacle and, later on, sways or even falls when he attempts to turn quickly. In the upper extremities the precision in delicate movements fails, and soon he cannot button his clothing, the handwriting is shaky, irregular and erratic, and finally the capability of writing at all is lost. The affection of the extremities is paretic, general, and progressive, and these are its characteristic features. Whereas in the syphilitic cases the extremities are most often affected (if affected) by unilateral or localised paralytic disability, of irregular course and sometimes temporary duration, and neither general nor progressive, but usually of the nature of an incomplete hemiplegia or partial hemiplegia, or of a paraplegia. But these may be associated with a slowness and torpor of movement generally, or an increasing weakness of limbs, or even a generalised motor paresis, the outcome of unusually generalised cerebro-spinal changes of syphilitic origin. Thus it approximates the motor condition of general paralysis, and, on the other hand, an approach may be made to the syphilitic cases in question by an early hemiplegia or paraplegia in general paralysis-dementia. The cases of which I have previously given summaries are illustrations of such an approach, and were partly chosen for that reason, this, with other symptoms, bringing them nearer and nearer in appearance to the syphilitic cases. Thus, by mutual approaches to a condition intermediate between the two conditions ordinarily present, may cases of these distinct affections be brought to resemble each other closely as to the motor condition of the extremities. The generalised motor impairment may, therefore, be very similar in the two diseases, and here it is that the contemporaneous complete paralysis of one or of several cerebral or spinal nerves aids greatly in the diagnosis of syphilis.

In general paralysis the electro-muscular contractility (faradism) of the extremities becomes considerably and progressively lessened, especially in the flexors of the feet; in the syphilitic cases it more frequently is normal or not much impaired. With this also the reflex activity of the limbs, especially of the lower, is lessened in general paralysis, but not until the middle and later stages. It may be added that in the later stages the patient pulls surrounding objects to or pushes them from himself, and resists passive motion.

In both affections temporary hemiplegias may occur, setting in with or without epileptiform or apoplectiform seizures, and in some

instances the onset and course of the hemiplegias are precisely alike. In the syphilitic, however, far more frequently than in the general paralytic, they may be due to thrombosis of (syphilitic) cerebral arteries, and hence of a more permanent nature, when the arteries are terminal. The distinction must be sought for in the other preceding or associated symptoms. Partial hemiplegias or other more localised paralyses occur more frequently in syphilis of the nervous system than in general paralysis, and the occurrence of successive paralyses in different parts would go far to decide in favour of multiple syphilitic lesions rather than of general paralysis. The paralytic affections are often earlier than any mental symptoms in syphilis, and the hemiplegias do not usually clear up so rapidly as they often do in general paralysis; while palsy of one or several cranial nerves, convulsions, or anæsthesiæ often coexist with the hemiplegia of syphilitic origin.

Contraction and rigidity of the limbs may occur in both; late in the demented general paralytic; either early or late in the syphilitic.

Convulsive Symptoms.—Unilateral convulsions, or local spasms without loss of consciousness occur not unfrequently in the syphilitic cases. But the same is true of general paralysis, and it is only when associated with optic neuritis, &c., that these would be of any value in enabling one to arrive at a diagnosis of material syphilitic lesion. In both they may occur in bouts of rapidly recurring, successive convulsions or spasms, and more general and severe convulsions with loss of consciousness may take place in both. But epilepsy is often an *early* or the first symptom in brain syphilis, and may continue later. In general paralysis this is far less frequent. There are the instances in which the first mental symptoms in syphilis distinctly follow upon epileptic or apoplectic seizures. But it is often distinctive that in syphilis headache, vertigo, &c., often continue between the convulsions, and that palsies of individual cranial nerves not unfrequently occur about the same period. Vertigo, again, is more frequent in intra-cranial syphilis, though it also may be found at an early period in general paralysis.

Tremulousness of the limbs and head or vibratile movements, even to the extent of a symptomatic paralysis agitans, may occur in both affections; in both are not usually permanent in their more marked degrees, and do not constitute elements of differential diagnosis between them.

Sensory symptoms.—Even in the late stages of ordinary general paralysis, including its dementia form, the superior sensory centres seem to maintain their reflex irritability. The patient starts back and blinks when a darting movement is made towards his eyes, or he starts at a sudden touch or sound. This is not seen in the syphilitic cases, and is in contrast with the cutaneous anæsthesia of

the limbs, especially of the lower limbs, and the lost or lessened excito-motor sensibility of the same parts, so often seen in advanced general paralysis. Early cutaneous hyperæsthesia and hyperæsthesia of cranial nerves may occur in both affections. Loss of sight or hearing, frequent and perhaps early in syphilitic cases, is rarer and usually later in general paralysis. Sudden or very rapid loss of sight or of hearing is strongly indicative of syphilis, as compared with the latter. Anæsthesia is often early and local in the syphilitic ; late, general, and progressive in the general paralytic. Moreover in the syphilitic it is sometimes sudden and complete in the parts it affects, and oftentimes is associated with paralysis of corresponding or of wider distribution. These local anæsthesiæ are highly distinctive of syphilis. In both, severe "rheumatoid" or "neuralgic" pains may afflict the limbs, neck, &c., or internal parts, in the early stages. In the syphilitic they may occur also in the later stages, and may be associated with obvious syphilitic osseous or periosteal disease, or with a local subacute or chronic spinal meningitis. The cephalalgia to which the patient is liable, and, perhaps, for a long period, is one of the most characteristic symptoms in constitutional syphilis. Typically it is very severe, is persistent or frequently recurs, is worse at night or occurs at night only, being brought on or increased by the warmth of the bed, is increased by pressure, is deeply seated, fixed, often quite local, and often is accompanied by insomnia, vertigo, or severe pains in several bones of the extremities. When wearing these characters it is a most important element in diagnosis. Yet in the form of syphilitic mental disease now under notice it often loses its special character or severity, or disappears, and no clear history of it can be obtained from the patient. Thus, the most striking syphilitic disorder of sensation may be virtually wanting in syphilitic dementia, and, on the other hand, in the early stages of the various forms of general paralysis severe protracted cephalalgia and various neuralgiæ may be present, and thus the available clinical distinctions in this aspect may be obliterated. But specific treatment usually relieves the syphilitic pain very much more readily and completely than that of general paralysis, for the relief of which, indeed, it is often quite inert.

Apoplectiform seizures are often more severe, and sometimes occur earlier, in the syphilitic cases, leaving behind them, also, paralyzes which are more or less permanent. The apoplectiform symptoms of general paralysis frequently recur from time to time and pass away rapidly, leaving behind them usually only some increase of mental impairment and of motor paresis.

Othæmatoma would be an evidence of general paralysis in these cases.

The actual coexistence of some of the tertiary lesions of syphilis

would afford some favour to the view that the mental malady was of syphilitic origin; and the test by specific treatment must never be forgotten.

2. The forms of confirmed syphilitic mental disease and of general paralysis, which most frequently resemble each other, have now been considered at length. The syphilitic cases in question would much more readily be distinguished from the better known form of general paralysis. The exalted delusions, the peculiar grandiose delirium, the intense self-satisfaction, and gay expansive state of self-consciousness, associated with an extreme and progressing emotional feebleness and facility, with the peculiar affection of speech, tongue, face, &c., and general progressive motor paresis would, far more easily than the *dementia* form, be recognised as general paralysis, even in the case of a patient where there was the history, or other proof, of constitutional syphilis. Or, again, it would be far more easy than in the *dementia* form to decide against the view of a syphilitic mental disease and in favour of general paralysis where the emotional state and the nature of the delusions rapidly changed, where the patient was gaily recounting his adventures and wealth one day, and on the next was plunged in hypochondriacal woe, or was profusely lachrymose and whining, in peevish distress, or in childish terrors, or dull and stupid, or sullen, irritable, and morose. Besides these rapid variations in the mental state, maniacal outbreaks are more frequent in this than in the *dementia* form of general paralysis. But maniacal and delirious outbreaks may occur also in syphilis. The following marks help to distinguish the maniacal seizures of general paralysis from those of encephalic syphilis:

1. In general paralysis a maniacal attack of some weeks' duration often occurs at the outset, supervening upon certain premonitory symptoms, especially such as restless activity, acts of extravagance, a wandering about, or squandering of means, or the entertaining of large and speculative schemes. 2. The motor paresis is more marked after the short maniacal attacks which may occur throughout the course of the disease independently of any convulsive or painful affection. 3. During these paroxysms the exaltation of feeling sometimes reaches its acme. 4. They are often attended by a rise in temperature.

The better recognised form of general paralysis, therefore, is separated by striking mental and physical symptoms from the cases of intra-cranial syphilis discussed in the previous portion of this paper. But again a difficulty arises, inasmuch as in some cases of intra-cranial syphilis simulating general paralysis, extravagant notions and exalted delusions occur; while the gait is tottering, speech and writing are impaired, and there is an inability to dress, and a blurring of the lines of facial expression, somewhat as in

general paralysis. Without contrasting at length this form of syphilitic disease, and general paralysis with exaltation, and thereby recapitulating much that has been written above on the dementia form of general paralysis and the syphilitic disease most like it, we may at once say that when exaltation of feeling and exaggerated or extravagant notions arise in the syphilitic cases they are distinguished—

1. By the distinct history or symptoms of syphilis.
2. By the preceding cranial pains, nocturnal and intense.
3. The exaltation is less marked, less persistent, and perhaps less associated with general maniacal restlessness and excitement, than in most of the cases of general paralysis.
4. Sometimes by such complications as palsies of one or several cranial nerves, or hemiplegia, paraplegia, &c., having the character and course of syphilitic palsies, adverted to above.
5. By the greater frequency of optic neuritis, early amaurosis, deafness, local anæsthesiæ, vertigo, or local rigid contraction.
6. The affection of articulation is paralytic rather than paretic, and usually speech is not accompanied by any facial or labial tremors.
7. By cerebral or spinal meningitis, or pachymeningitis.
8. By the variety of the motor and sensory symptoms, their capricious association or succession, and transitory character; and by the absence of the general progressive muscular paresis of the other disease.
9. By the effect of anti-syphilitic treatment.

Finally, there are cases of general paralysis where, mingled with the ever present and characteristic psychological weakness, the most prominent mental symptoms are those of a hypochondria or of a melancholia. Syphilitic cases simulating this form of general paralysis are distinguished from it by many, or all, of the non-mental symptoms already detailed above. Also by the circumstance that the hypochondriasis or the melancholy of syphilitic cases occurs at the outset; while, if there is a hypochondria or a melancholia in general paralysis, it occurs mostly during the middle periods, though occasionally at the onset. If at the onset, the symptoms usually are terrifying delusions; the patients say they “are to be shot,” or “are hunted,” “insulted,” “persecuted.” They are anxious, querulous, worried, and bewildered, distressed by trifles, suspicious of harm, sometimes have hallucinations of sight or hearing, or are either suicidal or hypochondriacal. These symptoms may continue even to the late stages. If occurring for the first time during the middle stages, the hypochondria coexists with a shattered understanding, and the delusions are usually of the following nature:—The patients say of themselves that they “are dead,” “have no head,” “have no throat and cannot swallow,”

"the intestines are gone," their eyes have been extirpated," "their testicles are wasted," or they "have been castrated," or "part of the body is dead." With this there is often a refusal of food and obstinate resistance to feeding, a wild howling when they are interfered with, an inability to walk or stand, a huddling up in bed, and general spasmodic tremor when locomotion is attempted, or when they shrink from contact. Though the above is a characteristic condition not found in intra-cranial syphilis, yet the diagnosis must usually be determined by certain of the points referred to previously as distinguishing the syphilitic cases from general paralysis with *dé lire ambitieux*.

Temperature.—The use of the thermometer may sometimes aid in the diagnosis in doubtful cases. The temperature in general paralysis has a tendency to a certain course and fluctuation. Five years ago I published results based upon a large number of thermometric observations in general paralysis, carefully collated with the coexisting physical and mental conditions and changes, and of which I quote the following summary :

"In the middle and later stages of general paralysis,

"1. A rise in temperature often accompanies a maniacal paroxysm.

"2. A rise in temperature often precedes and announces the approaching congestive or convulsive seizures, and nearly always accompanies them.

"3. When these states are prolonged (congestive or maniacal), the associated elevation of temperature is usually prolonged also.

"4. Defervescence of temperature, after its rise with excitement or apoplectiform attacks, often precedes the other symptoms of toning down to the usual state.

"5. Moderate apoplectiform attacks, or moderate maniacal exacerbation, are, however, not invariably associated with increased heat of body.

"6. A transitory rise in temperature may occur without any *apparent* change in mental or physical state to account for it.

"7. The evening temperature is usually higher than the morning temperature in general paralysis, and an absolutely high evening temperature occurs in cases rapidly progressing towards death.

"8. A *relatively* high evening temperature seems to be of evil omen, even when *not absolutely* very high.

"9. Rapidly progressing cases may show temperatures above the average, both in the morning and evening, for a long time before any complication exists; but (10) gradual exhaustion may pass on to death, in general paralysis, with the *average* morning temperature normal, or nearly so, throughout, except when raised temporarily by the special attacks to which general paralytics are subject."¹

¹ 'Journal of Mental Science,' April, 1872.

Pulse and Heart.—In the earlier stages of some cases of general paralysis the pulse is full and hard, the beat of the heart powerful, the first sound clear and sharp or full, the second accentuated, and the signs generally indicative of increased arterial tension. This, however, is *very far* from always present, or at all persistent if present, and though the arterial tension is usually lessened in cases of cerebral syphilis simulating general paralysis, yet it may be increased by other pathological conditions not always easy to detect in the demented condition of the patients. Thus the state of the heart and pulse usually ceases to be of practical value as a test, however interesting a place it may sometimes hold in the natural history of general paralysis.

The Morbid Anatomy.—Though the two diseases overlap each other in this respect, the lesions are sufficiently distinct in the well-marked cases. I do not now refer to all the intra-cranial organic changes which syphilis may produce, but merely to those of them which are most common in the particular groups of cases which simulate general paralysis, and with which alone we have been concerned throughout this paper. In these, then, are found lesions of the upper and lateral surfaces, with or without lesions of the base of the brain. There are found gummatous infiltrations of the meninges or cortex, in the latter with local induration of cerebral substance; gummatous nodules or tumours causing, perhaps, actual destruction of cortical grey matter; often the arterial changes, thickening, nodulation, &c., which were described in full in Part I¹ of this contribution, together with certain changes in the arterioles of the meninges and brain, leading to wasting of the cerebrum and to degeneration of cortical nerve-cells or thrombosis of terminal cerebral arteries and consequent softening of portions of the brain; or a diffuse exudation invading the cortex of the brain, and glueing it to the membranes, and perhaps these to the calvarium; or a simple or gummatous meningitis, especially at the base; or, according to some observers, an arachnitis with small-celled exudation, distinguished from the fibrous products in general paralysis. Pachymeningitis, internal and external, caries of skull, or pericranial nodes, may coexist, but have not an intimate relation to the particular form of syphilitic mental disease now under examination. Certain less frequent meningeal changes are common to it and to general paralysis.

Turning to general paralysis, we find in it also changes in the walls of the minute vessels, from dilatation to obliteration even of some of them, and consequent, or at least concurrent, malnutrition and degeneration of brain. But the microscopical appearances of the vessels, &c., are different, although, as this part of the subject is

¹ 'Brit. and For. Med.-Chir. Rev.,' July, 1876, pp. 168—173.

only partially worked out, it is better to refer to the naked-eye differences instead of the microscopical.

In general paralysis there is a decided tendency to atrophy of brain, with increase of subarachnoid fluid, œdema of meninges, and increased size of, and fluid in, the ventricles, the lining membrane of which is greatly thickened and often granulated. The medullary substance is often atrophied, discoloured, increased in consistence, or having the "gruyere" appearance; the grey cortical matter is often discoloured, wasted, degenerated, and, owing to softening of certain of its layers and to its adhesion to the meninges, it usually happens that its superficial layers strip off from the summits of the gyri along with the meninges when the latter are removed. Local, or more or less general, induration of the grey cortex is also often found. Then the meninges are usually thick, opaque, tough, and sometimes œdematous, or minute granulations are found on the arachnoid over the convexity. Arachnoid cysts are occasionally found. The spinal cord is often much altered, both as to its meninges and its substance, in the latter of which there are principally nerve-tissue atrophy and connective-tissue hyperplasia; or, again, the anatomical characters of chronic myelitis. Grouped and associated alterations such as these, without other and unequivocal intra-cranial syphilitic lesions, cannot be attributed to syphilis.

Treatment.—Aid may sometimes be obtained from the vigorous employment of mercurials and iodides, which have no beneficial effect upon general paralysis except sometimes for the relief of painful symptoms.

But when we have endeavoured to point out the principal differences between general paralysis and the group of cases of intra-cranial syphilis which are most like to it, we must still confess that, in their general character and outlines, there may be a very close symptomatological resemblance, and that it is sometimes necessary to withhold judgment for a period. We look at the widely spread wasting and degeneration of brain and cord in general paralysis, and upon turning to one class of syphilitic lesions we find also widely spread degeneration, or even some wasting or destruction of the cerebrum or cord. It is not merely that inflammatory products or gummatous masses or infiltrations displace or destroy brain substance; the source of the supplies is attacked, for not only is there syphilitic cachexia, from which the brain suffers as well as the rest of the economy, but its nutrition may be thoroughly undermined and its tissues degenerated by the widely spread syphilitic changes which sometimes occur in the walls of the arteries and of the arterioles, leading even, in places, to occlusion of the arterioles, or to the blighting effects of thrombosis of the larger vessels. Practically, therefore, the central nervous system may be deteriorated to the same degree by the two diseases, the work of destruction pro-

ceeding by different routes, and attended in its course by more or less dissimilar manifestations. In both these particular cases do we find the great central fact of a widely spread *progressive degeneration* of nervous tissues.

In conclusion, as the two diseases may be clinically much alike; as their symptoms even then differ somewhat in themselves, and more in the manner of their combination, succession, and duration; as the distinction is important in relation both to therapeutics and prognosis; and, finally, as the question is one of scientific interest, I have here stated in part the results of my own clinical studies, in the hope that they may not be altogether without practical value.

Chronicle of Medical Science.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By ROBERT HUNTER SEMPLE, M.D.,

Fellow of the Royal College of Physicians of London, Physician to the Bloomsbury Dispensary, London.

On the Value of Venesection. By Dr. TRASTOUR, of Nantes.—Dr. Trastour, whose practice lies in a French provincial town, asks the question whether medicine in modern times can dispense with the necessity of bleeding? In Paris, he observes, this measure has for several years fallen into complete discredit, although a few physicians of the younger school still have recourse to it. In the provinces, however, bleeding is not abandoned, for the population there, being more healthy and vigorous than in Paris, permits and encourages the practice. Dr. Trastour admits that many agents may be successfully employed instead of bleeding, such as sulphate of quinine, wine, coffee, alcohol, tartar emetic, calomel, alkalies, aconite, digitalis, &c.; but he nevertheless thinks that the omission of venesection often deprives the practitioner of a safe and rapid means of cure, and he offers some observations, founded on his own personal experience, in support of this view. In the first place, however, he declines to lay down any general rule on the subject, and considers that every case should be treated according to the peculiarity presented by the individual. In eruptive fevers, erysipelas, typhoid fever, &c., he agrees with those who abstain from bleeding altogether; but in acute pulmonary congestion, in pleurisy, and in some cases of acute dropsy, he believes bleeding to be occasionally very useful. He has himself lately attended a nun of eighty years old attacked with pneumonia, and apparently on the point of death, and who, it appeared, had been frequently bled. Dr. Trastour bled her again, and she subsequently untied the ligature, thus renewing the flow of blood. The improvement was so remarkable that a year afterwards she begged to be bled again, for fear of a fresh attack of pulmonary inflammation, with which she seemed to be threatened. Dr. Trastour also refers to some cases at the Salpêtrière, where females of eighty years old and upwards were bled; and out of sixty-five cases of pneumonia thirty-one were bled, of whom eighteen recovered and thirteen died, which he thinks not a bad result under such circumstances.—*Bulletin Général de Thérapeutique*, Sept. 15th, 1876.

On the Topical Use of a Hot Solution of Caustic Potash in Faucial Diphtheria. By Dr. F. P. MANN, of Brooklyn.—Dr. Mann has recently employed, in diphtheria of the back of the throat, liquor potassæ heated to about 150° Fahr., or as hot as could be taken into the mouth without pain. This fluid he applied to the exudation by means of an ordinary probang covered with a thin layer of cotton wool, and it was used three times a day, the patient in the interval, if old enough, being made to gargle the throat every hour with liquor potassæ diluted with an equal quantity of hot water, or, when the mouth was very sensitive, one part to three of water, at as high a temperature as could be borne without pain. The age of the patients so treated ranged from six to forty-five years. In every case after the second or third day Dr. Mann found the false membrane either detached or easily separated from the mucous surface by the probang, sometimes aided by common dressing forceps. The subjacent surface thus exposed was then treated by a gargle containing corrosive sublimate and hydrochloric acid in water and lemon syrup. Quinine was given internally, sometimes with tincture of iron. In explaining the beneficial effect of liquor potassæ, employed as above stated, in diphtheria of the fauces, Dr. Mann, while admitting the inutility of theorising on the etiology of the disease, refers to the microscopical characters observed in the false membrane, in which there are transparent, granular, round, and oval particles, bearing a certain resemblance to the germinal matter liberated from the cells of *Penicillium glaucum*. He conceives that the potash dissolves and perhaps decomposes the viscid substances which glue the diphtheritic exudation to the mucous membrane, and that it also dissolves the germs themselves. He has found experimentally that solution of potash kills the germs of *Oidium albicans* and *Penicillium*, which had been planted on various substances under favorable conditions for rapid propagation.—*New York Medical Journal*, Oct., 1876.

On the Employment of Cubeb in the Catarrhal Type of Diphtheria. By Dr. BEVERLEY ROBINSON, of New York.—Dr. Robinson, after some preliminary remarks on the relations existing between ordinary catarrhal affections and diphtheria, states that the idea of employing the balsamics in the latter disease originated with M. Trideau, of Andouille, who first used a mixture of styrax and copaiba, afterwards syrup of copaiba with cubeb, and more recently cubeb alone. This physician endeavoured to introduce this mode of treatment in Paris, and it was adopted by Trousseau, Paul, and Bergeron, but with only partial success. Dr. Robinson was house-physician in Bergeron's wards when the experiments were being conducted, and he believes that sufficient reasons can be given for the failures in many cases. He has himself made use of cubeb in about twenty cases of membranous sore throat, and of these he considers that eight or ten were examples of real diphtheritic infection. Two of the latter class of cases died, but he gives two other cases, very briefly recorded, in which the treatment was successful. It must be observed, however, that these two cases are not very conclusive,

for the details are very imperfect, and in the first case, which was a slight one, a mixture of iron and glycerine was applied to the fauces, in addition to the internal use of cubeb in powder. Dr. Robinson considers that cubeb, by stimulating the mucous surfaces, acts beneficially in diphtheria, both by direct contact, where it can be effected, and by elimination, which takes place in great part through the respiratory mucous membrane and through that of the kidney. He refers to a paper written by Dr. Reignier, of Surgères, a few years since, relating the successful treatment of a case of nasal diphtheria in a boy of ten years old, in whom also the urine was enormously loaded with albumen. It was in order to check the drain thus caused to the system that Dr. Reignier prescribed the cubeb, and the success exceeded his expectation not only in this case but in seven others of nasal diphtheria.—*American Journal of Medical Sciences*, July, 1876.

On Phenicated (Carbolised) Camphor and its Therapeutical Applications. By Dr. SOULEZ, of the Hospital of Romorantin.—Phenic acid and camphor are two powerful antiseptics, and Dr. Soulez, desiring to combine the properties of both, has devised a compound in which the camphor is dissolved in an alcoholic solution of phenic acid. The proportions he uses are, two and a half grammes (a gramme is about fifteen grains) of camphor in powder to one gramme of the acid, and the resulting compound is an oleaginous liquid of a yellowish pale colour, of a weak camphor smell, and not presenting, in any respect, the disagreeable odour of phenic acid. This liquid does not mix either with water or glycerine, but mixes in all proportions with olive and almond oils. In the preparation of dressings, Dr. Soulez employs mixtures containing one twentieth part of the phenicated camphor, either with olive oil or infusion of saponaria (soap-wort), which latter has the property of forming with the phenicated camphor a kind of emulsion. This dressing for wounds is said by Dr. Soulez to fulfil all the favorable conditions presented by the methods of A. Guérin and Professor Lister, without their inconveniences. In order to prove the value of this application Dr. Soulez refers to the state of the wards of his hospital and the prevalence in them of septic complications before the adoption of the phenicated camphor dressings, in comparison with the results of the new treatment. The hospital had been crowded with wounded patients after the battles at Orleans, and fatal results often ensued from a variety of secondary maladies due to the poisoned air of the establishment. By a thorough course of sanitary measures, however, all the septic diseases had been made to disappear except erysipelas, which continued its destructive course, often carrying away cases which seemed to be going on quite favorably. In this state of affairs Dr. Soulez adopted the phenicated camphor dressings, and the results were so remarkably favorable that he thinks himself justified in believing that the epidemic in question, namely, erysipelas, was completely checked by their instrumentality.—*Bulletin Général de Thérapeutique*, Aug. 15th, 1876.

On the Treatment of Typhoid Fever by Cold Baths, considered in reference to a recent Epidemic in Paris. By Dr. MAURICE RAYNAUD, of the Lariboisière Hospital, and others.—In the autumn of last year a very severe epidemic of typhoid fever visited Paris, and at the period when Dr. Raynaud's lecture was given there were twenty-six persons suffering from it out of eighty, the whole number of the lecturer's beds. He long hesitated before giving a decided opinion on the value of cold bathing in the treatment of this fever, but he is now convinced that it is an excellent remedy when judiciously employed. He does not know of any remedial measure which, in certain cases, gives such successful results. Excluding from the category all cases but those of true typhoid fever, the percentage of cures has been very remarkable for several years, and in the last year, up to the time when the report was made, there were fifty-four cases and eight deaths. But Dr. Raynaud does not by any means believe that this mode of treatment is so successful as some authors have asserted it to be, nor does he believe that in certain cases it is free from danger. His conclusion is, that in some cases the cold baths ought to be employed, but omitted in others. The existence of pneumonia and syncope constitutes an objection to the cold baths, and when such complications arise he thinks that tepid baths are preferable. With regard to the period when cold baths may be usefully employed in typhoid fever, Dr. Raynaud agrees with most authors in thinking that the sooner in the disease they are adopted the better will be the results obtained. The adoption of the cold bathing at the very outset of the disease depends upon the temperature of the patient; if it reaches forty or forty-one degrees (Cent.), and the symptoms are otherwise urgent, Dr. Raynaud immediately orders the baths, but if the temperature is only from thirty-nine to forty he waits for a little time.

This method of treating typhoid fever has been discussed in a recent meeting of the Société des Hôpitaux in Paris, when the subject was opened by Dr. Féréol, who related the results of his own experience. He appeals to statistics in favour of the treatment by cold baths, and he states that in 1873, when typhoid fever was treated by him in the usual manner, the deaths were at the rate of 27 in the 100; but in 1876, when the cold baths were freely employed, the mortality was only 15 in the 100. In 1875, when the cold baths were least resorted to, the mortality was the greatest. It is admitted, however, that statistical results cannot always be rigorously exact, because some cases are of a mild character, in which a cure is the rule rather than the exception. Still, when the results of treatment by the cold baths and by ordinary treatment are compared in the severe cases only, the result, according to Dr. Féréol, is decidedly in favour of the former. In replying to the objections offered to the employment of this mode of medication, he observes that the supervention of hæmoptysis, which has been attributed to the use of cold baths, seems rather due to the present epidemic constitution than to the treatment, and that one of his patients had hæmoptysis who had not been subjected to the cold bath. With

regard to other pulmonary complications, he does not believe that they constitute any objection to the cold-bath treatment, and that many persons so affected have recovered. As to hæmorrhages in general, Dr. Féréol does not deny that they seemed to him to be more numerous and the mortality greater in the treatment by cold baths than in that by other methods.

In briefly summing up the results of his experience, Dr. Féréol conscientiously believes that among the deaths there is not one which clearly appears to him to be attributable to the cold-bath treatment; while, on the other hand, among the cures there appear to him to be many which are exclusively due to this method.

Dr. Libermann drew attention to the dangers which might supervene from the treatment by cold baths in patients liable to laryngeal disease, and stated that of five cases which had been fatal three were the subjects of this complication.—*Ibid.*, Nov. 30th, Dec. 15th and 30th, 1876, and Jan. 15th, 1877.

On the Employment of Tepid Baths compared with that of Cold Baths in the Treatment of Typhoid Fever. By Dr. DUJARDIN-BEAUMETZ, of the Hôpital Saint-Antoine.—While admitting that the evidence of statistics is not yet conclusive as to the value of cold baths in typhoid fever, Dr. Dujardin-Beaumetz desires to call attention to the favorable results which may be obtained by a milder system of treatment. He refers, in the first instance, to the considerable mortality which is admitted to attend the cold-bath treatment, and to the serious symptoms sometimes induced, and he therefore argues that this kind of medication ought certainly by no means to be exclusively employed in typhoid fever, but should be reserved, as the advocates of this method admit, for the most serious cases. The most important features of such cases are the increase of temperature and the disordered condition of the functions of the nervous system, but he asks whether these conditions may not be relieved by other means than the cold bath, which he regards as a cruel and painful expedient. In lowering increased temperature, the importance of which in typhoid fever Dr. Dujardin-Beaumetz thinks is perhaps exaggerated, tepid baths have an action almost as powerful as that of cold ones, and in proof of this statement he refers to the data furnished by experimental researches. The tepid baths, unlike the cold, are always well borne; and while it is necessary to force the patients into the latter, the former are almost always desired by them. The comfort and relief experienced after the tepid baths have often been remarked; the skin becomes cool and supple, and resumes its usual moisture; the pulse is slower and more regular, and its dicrotism is less marked; the tongue becomes less dry, the thirst less intense, excitement is allayed, and sleep supervenes. In addition, the sedative action of tepid baths on the nervous symptoms is well known, and Esquirol and Brierre de Boismont have recommended them in delirium and the acute forms of mania. With tepid baths, the serious symptoms and complications often attending the cold ones are not to be apprehended, and even the most marked pulmonary affections do not constitute a contra-indication to their em-

ployment. Dr. Dujardin-Beaumetz concludes his paper by observing that the cold-bath treatment, whether rigorously followed out or subjected to special indications and counter-indications, has not given him any better results than the ordinary methods, and he therefore prefers the use of tepid baths.—*Ibid.*, Jan. 30th, 1877.

On the Employment of Salicine, Salicylic Acid, and the Salicylates, in the Treatment of Disease. By Dr. MACLAGAN and others.—The effects of salicine and of salicylic acid and the salicylates in the treatment of acute diseases are still the subjects of discussion; but, on the whole, it would appear that the very favorable results attributed to these agents have scarcely been confirmed in their fullest extent.

Dr. MacLagan announced in the early part of last year that salicine was a valuable remedy in rheumatic fever, the good effects being the more marked in proportion as the disease was more acute, the improvement being generally manifested in twenty-four hours, and always in forty-eight hours after the administration of the remedy; and he went so far as to state that salicine seemed sometimes to arrest the course of the disease as efficaciously as quinine cures intermittent fever. He also observed that the relief of pain was always one of the first effects produced, and that in the acute cases this improvement and the fall of temperature generally occurred together, though in the subacute cases the pain was sometimes sensibly relieved before the temperature began to fall. In a second paper on the same subject Dr. MacLagan arrives at the same conclusions; but he specifies the dose which should be given, and adds some remarks on the cardiac complications. The dose of salicine is thirty grains, given every two, three, or four hours, so as to saturate the system as quickly as possible, and the sooner this is done the more speedily are the fever and the pains subdued. With regard to the cardiac complications, he concludes that when the remedy is given sufficiently early and in sufficient doses these complications are prevented, and that its free administration is the best means of staying their progress after they have occurred, but that it is powerless to remove effusions which remain after the fever has passed away. He considers salicylic acid as having much the same action as salicine as an anti-pyretic and anti-rheumatic. The results obtained by Dr. MacLagan have been confirmed by some observers, but partially contested by others, and Sidney Ringer considers salicine as powerless in preventing cardiac complications. In typhoid fever salicylic acid and the salicylate of soda have been employed in Germany with some success. In some cases the administration of the medicine has been followed by troublesome symptoms, sometimes local and sometimes general, but not often very serious. Whether salicine is a specific against any disease remains to be proved, but it certainly seems to be serviceable in acute rheumatism.—*Lancet*, Oct. 28th, 1876; *Bulletin Général de Thérapeutique*, Jan., 1877; *British Medical Journal*, Feb. 24th, 1877.

On the Use of Chrysophanic Acid in the Treatment of Skin Diseases,

By Dr. BALMANNO SQUIRE and others.—Dr. Balmanno Squire having found chrysophanic acid useful in the treatment of psoriasis, a question has been asked by Dr. Foulis, now of Glasgow, but formerly residing in India, whether this acid existed in the Goa powder, and also in the leaves of the *Cassia lata* (or more correctly, we believe, *alata*), which latter Dr. Foulis had found very serviceable in a form of vesicular ringworm common in India. From the replies of various writers it appears that chrysophanic acid exists in Goa powder to the extent of 85 per cent., and that it forms $2\frac{1}{2}$ per cent. of the medicinal rhubarb root, and also exists in senna leaves, though in small proportion. It is not yet determined whether the other species of cassia besides the senna plant also contain this substance. With regard to the use of chrysophanic acid in ringworm, Dr. Balmanno Squire remarks that there are two kinds of this affection, differing very widely in their nature, one being an eruption of an ordinary kind, usually occurring in the form of rings, and the other being specially due to the presence of a parasite, the *Trichophyton tonsurans*. Having verified some of the latter kind of cases by microscopical examination, and having employed chrysophanic acid in their treatment, he has arrived at the conclusion that it is a true parasiticide. In replying to some communications recommending the use of Goa powder, Dr. Squire advocates in preference the chrysophanic acid itself, the composition of which is known, while that of Goa powder is a secret. The mode of application is also a matter of importance, and Dr. Squire thinks that the way in which the remedy is used in India, namely, by pasting on the powder with water or vinegar, is inefficacious. He recommends that it should be mixed with hot lard, which dissolves it pretty freely, 85 per cent. of Goa powder—that is to say, its chrysophanic acid—being thus dissolved; or, if pure chrysophanic acid be employed, a perfectly clear solution may be procured and an ointment obtained on cooling.—*British Medical Journal*, Jan. 20th, 27th; Feb. 3rd, 10th, 17th, 1877.

On the Use of Goa or Araroba Powder in Ringworm. By Dr. H. R. CROCKER, of University College Hospital.—Dr. Crocker having heard of the reputation possessed by the secret remedy in India for the treatment of ringworm, determined to put the substance in question to the test in treating the patients with skin disease in the practice of University College Hospital. The powder is known under various names, but its identity was first suggested by Dr. Lima, of Bahia, in 1875, and was subsequently pretty well proved by other writers. The botanical name of the plant from which it is derived is not known, but it has been referred to different species of *Centralobium* and *Cæsalpinia*, growing in Brazil. The most important fact in relation to the composition of this powder is that it contains 80 per cent. of chrysophanic acid, easily obtained by exhausting the powder with hot benzole, its best solvent, when the acid is evaporated out as a yellow granular powder. Dr. Crocker treated his first cases by painting on the patches of disease a saturated solution of chrysophanic acid in benzole, which retains ten grains to

the ounce in the cold. Other cases were treated with an ointment composed of chrysophanic acid, acetic acid, and simple ointment. After a few cases had been so treated Dr. Crocker received from Bahia a canister of the Poli de Bahia, which is identical with the Goa or Araroba powder. This powder was found to contain 82 per cent. of chrysophanic acid by analysis made in the hospital. Like other observers of the treatment of ringworm, Dr. Crocker draws a wide distinction between the ringworm of the body (*Tinea circinata*) and that of the hairy scalp, *Tinea tonsurans*, the former being far more easily curable than the other, whatever means are used. Dr. Crocker's conclusion from his experiments is that the powder is efficacious when the disease is on the body, but that it fails when the scalp is affected. It appears difficult to bring it into sufficient contact with the fungus of which the disease consists.—*Lancet*, Jan. 27th, 1877.

On the Treatment of Ringworm of the Scalp by Croton Oil. By Dr. LADREIT DE LACHARRIÈRE.—Dr. de Lacharrière, having endeavoured to discover a method of treatment for ringworm of the scalp without the necessity of removing the hairs, made trial of a great number of substances of the parasiticide order. These experiments appearing to him to be more or less unsuccessful, he asked himself whether an inflammatory process developed in the hair-bulbs and the hairy scalp might not draw out and destroy the morbid germs? After trying various substances with this view he believes that he has completely succeeded by means of croton oil. This oil applied upon the hairy scalp does not produce there, as it does on other parts of the body, the well-known abundant eruption in a few hours. It is only after about four and twenty hours that the effects are produced, and these consist, at first, of a slight swelling and a little sensitiveness, followed by vesication, the vesicles gradually uniting together and forming soft, yellowish crusts, having the appearance of concreted honey, and resembling the crusts of impetigo. When these are detached by poultices or lotions it is usually found that a great number of hairs are carried away, and that others are no longer adherent and may be removed without pain.

With reference to the practical application of this remedy, Dr. de Lacharrière finds that the best plan is to incorporate the croton oil with a semi-solid and easily fusible body which permits of the application being exactly localised, and with this object he has constructed some sticks made of cacao butter and white wax, containing 50 per cent. of croton oil. He commenced this treatment about a year ago, and since then eighteen patients under his care have recovered in a comparatively short time. The duration of the treatment was six weeks to two months in each case, but the disease had existed in most of them for more than six months, and in one case the child had been affected for two years, in another for three, and in a third for five years. In all of them the performance of epilation and the application of turbith mineral and of corrosive sublimate had been unsuccessful.—*Bulletin Général de Thérapeutique*, August 15th, 1876.

On the Use and Value of Arsenic in the Treatment of Diseases of the Skin. By Dr. L. D. BULKLEY, of New York.—In this paper Dr. Bulkley does not claim the merit of novelty, nor does he profess to give an exhaustive view of the uses of arsenic in skin diseases, but his objects are to point out the cases where this mineral is serviceable and where it is useless, to indicate the doses in which it may be used, and to specify the forms in which it may be most advantageously prescribed. In the first place he insists strongly on the necessity of accurate diagnosis as a chief element of success in treatment, because arsenic is by no means a panacea in skin diseases, and its use must be restricted to suitable cases. He then passes in review the different skin diseases in which arsenic has been proved to be efficacious, commencing with psoriasis (including in that term lepra), in which it was originally given, and for which it is now most commonly prescribed. Dr. Bulkley, however, limits the appellation of psoriasis to the disease known as such in the United States, Britain, Germany, and France, and the symptoms of which are well defined. This affection, as is well known, is often very obstinate, and resists even the administration of arsenic; but Dr. Bulkley considers that in the instances of failure some rules of treatment have been disregarded, or the drug has not been given long enough or in sufficient doses. He agrees with those authors who regard the use of this substance as quite harmless when given with due precautions, and he points to the positively beneficial effects produced on the general health and on the condition of the skin in particular by the practice of eating arsenic, as in Styria. In treating psoriasis, it is sometimes necessary to continue the use of the drug, perhaps for months, before a cure is effected, and it may be given to children as well as to adults with equal safety. In the treatment of eczema arsenic is less conspicuously useful than in psoriasis, and it often requires to be combined with other remedies, but, nevertheless, Dr. Bulkley considers it to be very efficacious in certain cases, and he gives an instance of a child of five years old affected with eczema, who took fifteen drops of Fowler's solution, or one eighth of a grain of arsenious acid three times a day, and for a long time, with beneficial effects. Dr. Bulkley goes on to specify the other cases in which arsenic may be usefully employed in the treatment of skin diseases and the methods of employing it, and the general results of his experience are very well summarised by himself in nearly the following terms.

Arsenic, when administered in medicinal doses, has a totally different action from that manifested in poisonous ones, and does not produce slow poisoning, as might be supposed, Hebra having given a total of more than half an ounce to a single patient without injury.

It is eliminated very rapidly, chiefly by the bowels and kidneys, and no trace of it can be discovered in the body after death two weeks after the last dose has been given. The first symptoms of a full dose of arsenic, in a very large number of cases, are a fullness about the face and eyes and conjunctival irritation and tenderness,

but these symptoms are unimportant and only show that the remedy has begun to affect the system. Arsenic is certainly valuable in psoriasis, eczema, pemphigus, acne, and lichen; of less certain value in lupus, ichthyosis, sycosis, warts, and epitheliomatous and cancerous diseases; and useless or injurious in the syphilodermata, the animal and vegetable parasitic diseases (except in rare cases), in elephantiasis Græcorum et Arabum, purpura, true prurigo, zona, scleroderma, molluscum, keloid, vitiligo, nævus, &c.

The dose of the drug should be small at first, and increased slowly until some of its physiological effects are manifested or the disease yields, and then it may be somewhat diminished. The most serviceable forms in which to use arsenic, named in the order of their value, are, solution of the chloride, solution of arseniate of potassa or of arseniate of soda or of arseniate of ammonia, arsenious acid, iodide of arsenic, the arseniates of iron and quinia, and perhaps the solution of the chloro-phosphide of arsenic, and arseniate of antimony. —*New York Medical Journal*, Aug., 1876.

On the Employment of Turpentine Baths in Rheumatic Cases. By Dr. BREMOND, Jun.—In a pamphlet under the above heading Dr. Bremond strongly advocates the use of turpentine baths, employed by placing the patient in an apparatus where his body is enveloped in an atmosphere saturated with turpentine.

The apparatus consists of a box placed in communication with a generator of steam, and it is so contrived that, at the moment when the steam enters, it is charged with turpentine, the latter being very finely divided into a kind of spray which permits of its ready absorption by the skin.

The patient is seated on an easy chair inside the box, but his head is outside, and is never exposed to the medicated vapours. The temperature of the bath does not exceed forty-five degrees Centigrade, and generally forty degrees are sufficient; and Dr. Bremond states that he has observed only twice, out of more than 3000 baths, any of that artificial exanthem which accompanies terebinthinate applications to the skin, and yet he employs for every bath more than 200 grammes (a gramme is about fifteen grains) of essence of turpentine, or rather of the essence of the Californian cedar, which seems to him more efficient than that of the indigenous turpentine. The medicated steam is spread over the surface of the body by means of the apparatus described, and is absorbed by the skin, and Dr. Bremond considers that the turpentine thus introduced into the tissues meets the residues of nutrition when these are in excess, and, by causing their combustion, prevents the undue formation of uric acid. He refers to another action of turpentine, namely, its effects on the tegumentary system, and he considers that the drug produces on the mucous membranes an effect quite as beneficial as that of its isomer copaiba.

On the Hypodermic Mercurial Treatment of Syphilis. By Dr. ISIDORE NEUMANN, Professor of Dermatology and of Syphilitic Diseases in the University of Vienna.—Dr. Neumann, in commencing his paper, expresses his astonishment that the hypodermic em-

ployment of mercury has been so little practised, notwithstanding its admitted value. The reason he assigns for this neglect is perhaps, he says, to be found in the serious consequences sometimes arising from the local changes caused by the injected solution, such as suppuration and scabbing of the skin, leading to fever, diarrhoea, suppuration of the mesenteric glands, and even to death. These results seemed to arise from the particular preparations employed, but since a form of solution has been devised which appears to produce no bad consequences, the only question now is whether the recent form of injection may be always employed without any danger. Lewin, of Berlin, first applied the hypodermic mercurial treatment on scientific principles, and some cases he cured by four injections and others not till after forty-two, but, on the average, the number of injections was between eighteen and twenty-eight. He recommended a certain mode of procedure in carrying out his plan, some of his rules being as follows. The injections are to be practised in the subcutaneous areolar tissue, and not in the muscles; after the injection the spot of the puncture is to be closed a little, in order to prevent the escape of the fluid and to secure its due distribution in the tissue. At first Lewin chose as the seat of injection the vicinity of the lymphatic glands of the axillary, inguinal, and cubital region as being nearest to the seat of the disease, but as he found that abscesses sometimes supervened in those parts he preferred the skin of the side, breast, pelvis, and buttock. Only in iritis was the temporal region injected. The dose employed was for each injection fifteen drops of a solution containing four grains to the ounce of distilled water, and, therefore, equivalent to one eighth of a grain; but in iritis even one fourth of a grain was employed, by which means a cure was obtained in from five to seven days. Tertiary symptoms require thirty to forty-two injections. The older the patient the more injections are necessary. Preventive injections delay the supervention of general symptoms and make them milder.

The preparation which has been most successful for injection is a modification of that devised by Bamberger, which contains a solution of albumen, corrosive sublimate, solution of salt, and distilled water; but in order to obtain a perfectly clear solution Dr. Neumann has invented a formula consisting of dried albumen dissolved in distilled water and mixed with the salt (chloride of sodium) and corrosive sublimate. The clearer the solution is, the more constant is the proportion of the corrosive sublimate to the solution of albumen; in a turbid solution, calomel separates as a white deposit in the vessel employed, and this form of mercury has no influence on syphilis. Dr. Neumann found that injections in the back and the lateral regions of the thorax were attended with much less reaction than when made in the extremities. He concludes his paper by giving details of a number of cases in which the mercurial injections were employed, and in many cases with remarkable success.—*Medizinische Jahrbücher von S. Stricker in Wien*, 1877.

On the Use of Erythroxyton Cuca in Typhus and Typhoid Fevers and also in others Febrile Diseases. By SAMUEL McBEAN, of Newcastle-on-Tyne.—Mr. McBean introduces his paper by observing that in all febrile diseases, with the exception of yellow fever, the amount of urea formed is abnormally increased, and, as was first observed by Dr. Ringer, the amount of this substance excreted stands in a certain relationship to the temperature of the body. Hence, as urea represents the wear and tear of the system, its amount may be regarded in fever as affording, not only an index to the severity of the disease but also a direct clue to a very important line of treatment. He goes on to argue that, as in fever there exists a specific morbid agency leading to an undue extent of tissue metamorphosis (as indicated by the increased quantity of urea excreted) it is desirable to restrain such metamorphosis until the morbid agency has ceased to exist. In the accomplishment of this object he believes that cuca is a very efficient agent, this substance consisting of the dried leaves of the *Erythroxyton cuca*, and having, when taken by a healthy person, the power of diminishing the excretion of urea. In this property, according to Mr. McBean, lies the value of cuca as a therapeutic agent. The author gives three cases in which the drug was successfully employed, although in the first of these the patient, after being convalescent from typhoid fever, died of a supervening attack of lobular pneumonia. In all the cases, the administration of the cuca appeared to be followed by diminution of temperature and relief of the symptoms, and in the first case the degree of temperature, the rate of the pulse and of the respirations, and the relative quantity of urea excreted are all accurately recorded. Mr. McBean has found cuca also serviceable in both acute and chronic pneumonic phthisis, and, where there is much febrile excitement, this drug lowers the temperature and restrains or materially alleviates the distressing perspiration. The modes of administration of the cuca are in the tincture (the dose being a drachm or a drachm and a half) or in infusion of the leaves, and Mr. McBean states that he has obtained the tincture from Messrs. Hewlett and Son, of Leadenhall Street, London.—*British Medical Journal*, March 10th, 1877.

REPORT ON PATHOLOGY AND THE PRACTICE OF
MEDICINE.

BY JOHN T. ARLIDGE, M.D., A.B. Lond., F.R.C.P. Lond.,

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Plessimetric Auscultation.—Dr. Noel Gueneau de Mussy has noted that when the ear is applied to the upper portion of the front of the chest, and percussion is made on the upper dorsal spinous processes, a peculiar metallic vibration is heard over and above the sound produced by the tap itself. The like phenomenon transpires when the sternum is percussed and the ear applied to the upper and posterior part of the thorax. The name proposed for this sound is “plessimetric transsonance,” and the process “plessimetric auscultation.”

This plan of exploration is not new, for Trousseau employed it for the diagnosis of pneumothorax, and noted the existence of a brassy sound as elicited in this malady by this mode of percussion. M. de Mussy, however, points out other clinical applications of the proceeding. For instance, when the pulmonary tissue is modified in density and permeability, leading to defective homogeneity of the medium through which the waves of sound are transmitted, the sound, which in the normal condition is vibrating and metallic, is rendered more dull and feeble, although its pitch is at times simultaneously raised. In this way central lesions of the lung which escape detection by percussing as ordinarily practised may be diagnosed, such as, for example, tubercular indurations at the apex, bronchial adenopathy, and centric pneumonia. At the level of the inferior lobes of the lung, the liver on the one side and the heart on the other arrest the sonorous undulations. From this circumstance we acquire a means of determining the upper limit of the liver and the situation of the heart, because on the level of either viscus there is a sudden change of the transresonance.—Hayem's *Revue des Sciences Médicales*, 1877, p. 130.

Renal Changes in Gout.—Litten relates a case of gout curiously occurring in an ill-fed pauper, in whom no hereditary tendency was discoverable. The patient rapidly succumbed to a well-marked attack of gout ushered in without any premonitory attacks.

The urine was clear but scanty and highly albuminous, without sediment or preformed elements. Considerable anasarca and fluid in the serous sacs; no cardiac lesion. The symptoms were not those of renal atrophy, and, in the absence of enlargement of the spleen and of diarrhoea, those of amyloid disease were also wanting. Nevertheless, the autopsy revealed very advanced amyloid disease, with simple atrophy. Such form of renal lesion has hitherto not been described in gout, but Litten believes it will not be found so rare if looked after, particularly in acute cases, because it has as yet

escaped notice by reason of the amyloid change being readily mistaken for simple atrophy of the kidney. In this same patient another very unusual fact occurred, viz. the presence of deposits of urates (tophi) in the larynx; however, as was still more remarkable, these deposits were unaccompanied by any alteration of the voice.—*Archiv für path. Anatomie*, Band lxvi, p. 129; and Hayem's *Revue des Sciences Médicales*, tome ix, 1877, p. 68.

Excretion of Oxalid Acid in the Urine.—Dr. Paul Fürbringer, of Heidelberg, has conducted a large series of experiments on the excretion of oxalic acid in the urine, and on the causes influencing its amount. The résumé of the conclusions whereat he has arrived is—

1. Oxalic acid is a normal and probably a constant constituent of the urine.
2. The excretion of oxalic acid in the urine rarely exceeds (where an ordinary mixed diet is used, devoid of excess in oxalates, and there is a fair amount of activity) 20 milligrammes per day.
3. The quantity of oxalate of lime contained in the sediment of the urine bears no necessary relation to the entire amount present in the urine, even when this has been allowed to stand for twenty-four hours. The difference between the two exists in solution. In fact, urine occurs in which no signs of crystals of oxalates can be discovered, even after it has stood twenty-four hours, but which, notwithstanding, is really richer in oxalic acid than other urine, in the sediment of which numerous crystals of oxalate of lime are visible.
4. The most active solvent for oxalate of lime in the urine is the acid phosphate of soda. The smaller the degree of the acidity of the urine, the closer does the proportion of the precipitate oxalate of lime approximate to the entire quantity of that salt existing.
5. The internal administration of bicarbonate of soda does not augment the proportion of oxalic acid poured out in the urine.
6. The ingestion of lime water in moderate doses does not increase the excretion of oxalic acid.
7. The introduction of uric acid salts does not necessarily increase the oxalic acid discharged with the urine.
8. There is no constant relation between the extension of oxalic acid in higher proportion with the urine and an arrest of the normal oxidation process in the organism.
9. Fever does not exclude the occurrence of a self-produced discharge of oxalate of lime with the urine.—*Deutsches Archiv*, Band xviii, p. 144.

The Pathology of the Accumulation of White Corpuscles in the Cortical Substance of the Brain.—Duke Charles, of Bavaria, who is a doctor of medicine, has instituted a series of investigations respecting the accuracy of the assertion of Popoff, that in typhoid fever and in the later stage of typhus the most noteworthy circumstance in the cortical substance of the brain is an accumulation of white corpuscles, not only in the vicinity of the ganglion-cells, but also within those cells themselves; and that, as a consequence, there ensues a tendency to the breaking up of the cell-nuclei. In these presumed morbid changes Popoff, moreover, found an explanation of the disturbed cerebral phenomena attending the diseases in question.

To fairly test the accuracy of Popoff's statements, Duke Charles at first followed out precisely his method of making the preparations of cerebral matter and of examining them, but experience soon showed him that this method gave very unsatisfactory and indefinite results. He consequently next resorted to other methods, which gave clearer insight into the matter in dispute, and convinced him that, although a collection of white corpuscles existed, none such were present within the ganglion-cells, and that the nuclei of the last did not undergo fission. He further proceeded with a much wider range of inquiries than Popoff, investigating the brain substance in fever at all stages, in a variety of general and local diseases, and in animals in a state of health. Altogether he examined fifty different brains and 500 preparations made from them.

The general result was, that these white corpuscles are always to be found in the cortical substance, but that their relative quantity varies in different brains and in different diseases, being greater in those maladies in which those corpuscles are produced in excess, and also in those wherein the circulation is retarded.

With reference to the position of the multiplied corpuscles, it is to be admitted that these bodies are especially collected around the ganglion-cells, and often press upon them, and, thrusting in their wall, are lodged in pouches so formed, but without gaining their interior. A second locality in which the corpuscles abound is the lymph-spaces of His. On the other hand, they are few or quite absent in the general substance of the brain or in association with neuroglia-cells, except, indeed, in cases of meningo-cephalitis; and the idea suggests itself that between the perivascular spaces and the circumference of the ganglion-cells a system of communicating canals is in existence. Obersteiner, indeed, advanced this opinion, and stated that he was fortunate in being enabled to inject the periganglionic space from the perivascular canals.

From the preceding observations the hypothesis may be hazarded, that the phenomenon of accumulation is due to an emigration of white corpuscles from the blood in circulation—a known result of inflammatory action. Moreover, it is an admitted fact that the multiplication of white corpuscles goes along with an increase of the aqueous constituents of the blood and with retarded circulation; and this holds true, not only with regard to cerebral matter, but likewise to other organs the subjects of the same morbid condition. Especially is this fact exhibited in the liver.

The *résumé* of conclusions is—

1. That in the cortical substance all brains, even of the healthiest, there is a considerable number of white corpuscles.
2. Retarded circulation and an increase of water in the blood bring about an increased production of white corpuscles.
3. These bodies exist in the perivascular canals of His, in the adventitial canals of Robin, and pre-eminently in the periganglionic spaces.
4. They never penetrate within the ganglion-cells themselves, and do not give rise to division of the nuclei of those cells.

5. The cerebral symptoms depend in no instance upon the presence of the white corpuscles, nor upon their multiplication, nor upon retarded circulation and an increase of watery constituents, when these constituents are of slow access and lingering; but they must be attributed to an acute augmentation of all three morbid conditions, wherein consequently a process of fever and the exciting cause of fever assume the character of factors.—*Virchow's Archiv*, Band lxxix, 1877, p. 35.

Chemical Discrimination of Alkaline Salts of Urine.—Dr. C. Stein, in a paper on alkaline urine and its occurrence in stomach disease, refers to the difficulty frequently experienced in discriminating between the several salts that occur in alkaline urine. He notes that dilute acetic acid is insufficient, and that, although ammonia acts upon the phosphate of magnesia, and apparently leaves triple phosphate and phosphate of lime unchanged, it does not clearly establish the difference between the phosphate of magnesia and triple phosphate, and fails, even after a long interval, to differentiate between triple phosphate and calcium phosphate. On the other hand, he finds a solution of commercial carbonate of ammonia, prepared with gentle heat, in the proportion of one part to five parts of water, an efficient reagent to distinguish between the three sedimentary deposits in question.

On placing under the microscope a small portion of each of the three substances, or on introducing a larger quantity in a test-tube, and adding the solution in question, it is found that the crystals of the triple phosphate remain unchanged, their angles and sides continuing as sharp as ever, and their transparency as decided. However, after submitting them to the action of the solution for a day their lustre dims, and a waxy appearance is assumed, the surface looking to be pitted by small holes; nevertheless, even after the lapse of seven days, some few crystals will still be found. On the contrary, the crystals of magnesium phosphate are immediately altered. The transparent clear laminae become forthwith dull, and in a few minutes the edges become jagged and the entire surface speckled. After forty-eight hours some clusters of crystals, looking like skeletons of their former selves, may be seen, and with them numerous very small angular crystals.

The phosphate of calcium, on its part, at once loses its sharp contour, yet does not dissolve and disappear, but seems to be transformed into a multitude of very small spheroidal particles, closely resembling the globules of *mycoderma aceti*. These particles cohere in masses, adherent to the object-glass, and may be collected from it unchanged, even after rinsing. They are readily soluble in hydrochloric acid, and after the lapse of several hours they assume an irregular figure, and after a longer interval the character of true crystals. It is true that similar small angular crystals result from the prolonged action of the reagent on magnesium phosphate, but these do not adhere to the glass, as do those of calcium phosphate, but are readily washed away by rinsing.

Further experiment showed that artificially prepared crystallised

phosphate of magnesia, when a long time digested with carbonate of ammonia, is completely transformed into triple phosphate, and that it is from the onset of this transformation that its crystals assume, on the addition of the latter, a shagreened aspect.—*Deutsches Archiv für Klinische Medicin*, 1876, Band xviii, p. 207.

Pulmonary Consumption in Colorado.—The merits of the climate of the very extensive State of Colorado in cases of phthisis have become largely recognised in this country. The report of the Colorado State Medical Society for 1876 contains some interesting observations on this subject, for the most part contributed by Dr. Lemen, of Denver. The State is to a large extent mountainous, and its more level tracts represent raised plateaus. The high range of the rocky mountains shelters a large area of the country, and, at the same time, causes a precipitation of much of the moisture contained in the warm currents of air which find their way from the Gulf of Mexico, before reaching the Colorado territory. The winds are in general infrequent and fitful, and if rapid, of short duration. In the wide region forming the State the character of the winds differs in different places, according to their position relative to mountain ranges and the valleys and the channels of rivers. Dr. Denison, who reports on the winds of Colorado, states that "the north, north-east and east winds, coming up from the north Missouri valley, bring most of the storms for north-eastern Colorado, and having lost their moisture push on to New Mexico, carrying dry weather to Santa Fé, or rising above the stronger southern current they give rain to the plains and pleasant weather to the south-western mountains." Again, he says, "The broad plains east of the mountains are almost barren of trees, have few rivers, are composed of a dry, sandy soil, and have a remarkably pure atmosphere overhead, which offers a minimum resistance to the caloric effects of the sun's rays. This portion, then, protected on the west and north-west by the mountain ranges, may be considered as drawing to itself the air of neighbouring regions to replace its constantly expanding and rarified atmosphere, the daily work of the sun. Thus, I think, a degree of warmth in this elevated country is explained, which could not otherwise be accounted for in this altitude and latitude."

To express in a few words the general characters of the climate, it may be described as deficient in moisture, stimulating or exhilarating. The soil generally is porous and absorbs the rain and snow almost as they fall.

Dr. Lemen wisely notes the extravagant expectations raised by the puffs of hotel-keepers and others interested in attracting invalids to the state, and has to lament the sad result in the destruction of numerous consumptives who ought never to have been sent from home. It is neither a climate for all forms nor for all stages of phthisis. To arrive at some general conclusions Dr. Lemen recounts and analyses the histories of forty-four cases, occurring in his practice. Hemorrhage occurred from the lungs in 76.19 per cent. of these cases, an unusually large proportion. This circumstance cannot be entirely attributed to the altitude of the country, since

inquiries showed that in a large majority of those who suffered and had come from a distance hæmorrhage had previously occurred. The percentage of laryngeal complications was 22·72, and consequently large, and, on the whole, Dr. Lemen regards the climate as unfavorable to this complication. Improvement took place in 43·18 per cent. of the case, and the average duration of the disease in the sixteen fatal cases was forty-two months. But it would seem that little more than one fourth of the period of duration of the disease was spent in Colorado, so that the climate did not have fair play. The general inferences are, that those cases will live longer at the sea-level, surrounded by ordinary salubrious conditions, than in the elevated region of Colorado, where one lung is *extensively* involved, or both to a less extent; in which the inherent tendency to dissolution is obstinate and continuous; the digestion and assimilation greatly impaired; the nervous system profoundly disturbed and depressed, or exceptionally irritable; the mesenteric glands involved; an unusually high pulse and temperature standard. For the same reasons an individual suffering from an attack of ordinary phthisis, but undergoing an exacerbation of cough, frequent pulse, dyspnoea, loss of appetite, weight, strength, would probably do quite as well to place himself under salubrious surroundings and well-directed medical treatment until acute accessions have been measurably controlled, and then seek a higher altitude and the climatic conditions it furnishes, as to rush at once to the latter when in an unfavorable plight to engage upon a long fatiguing journey, or to tolerate a considerable rarefaction of the air he breathes, associated with great daily variations of temperature, as most elevated and tainous regions are.

It is very common to attribute rheumatism and nasal catarrh to cold and moisture, but these maladies, Dr. Stedman tells us, are among the most prevalent in the dry air of Colorado, and are due to the great diurnal variations of the thermometer and the wide change of temperature found in the shade and in the direct rays of the sun. In this climate, again, the nervous symptoms of rheumatism are more pronounced, whilst, on the contrary, heart complications seem less common than elsewhere.

Further, pneumonia develops more rapidly, the exudation is more copious, and the tendency to invade the sound tissues consecutively much more marked than in the Atlantic states. Urgent dyspnoea, capillary stasis, asphyxia and death, come so rapidly in some cases that remedies have to be applied early and diligently to be of any avail, and, on the other hand, resolution sometimes takes place so rapidly and completely in a few hours, as to almost lead the practitioner to doubt the existence of the disease, had he not had proof furnished by the physical signs. In the acute and rapid form of the disease Dr. Stedman finds bloodletting the most useful treatment when applied early. Internally, he gives quinine and stimulants, and externally uses turpentine freely.

Case of Cystinuria, with comments.—The case occurred in the hospital practice of Professor Ebstein, of Gottingen, and is reported

by Dr. Trieman, who instituted a series of chemical investigations respecting cystin, and a comparative examination of the leading characters of cystinuria.

The patient was a youth, aged 18, an apprentice to a tradesman, and had previously suffered from no severe illness. His father had died in his forty-fifth year from liver disease. Of his sisters one died from typhus, but two were alive and in good health. The mother was also alive and well. For four years he had noticed tremors of his hands to no inconsiderable extent; these had rather increased, and interfered with his writing. He had also noticed at times, chiefly in the morning when in bed, stabbing pains in the renal region; these were, however, not very acute, and oftentime disappeared on turning himself in bed. But in the spring of 1875 he was seized with very severe pain in the left renal region, accompanied by vomiting, which confined him for three days to his bed and then vanished, leaving him apparently well. A few weeks afterwards, on desiring to pass water, he found he could not do so, even on straining, except in drops; this lasted for an hour, without actual pain, and then the urine flowed in a full stream. This state of things recurred eight days subsequently, and after powerful straining he passed a calculus, and immediately afterwards had a free passage for the water. The calculus came into the hands of Professor Ebstein, who discovered it to be composed of cystin, and admitted the patient into his ward on the 8th of April. He is recorded as being of middle stature, of weakly conformation, and pallid. When he outstretches his arms they exhibit a constant tremor. In his general condition nothing otherwise abnormal was noted. His liver was not enlarged and the stools were normal. The urine had a whitish sediment and natural reaction. Its specific gravity was 1019, and it contained no albumen. Its quantity was natural. The deposited matter, on microscopical examination, showed the presence of transparent, glistening, hexagonal laminar crystals, which, by their chemical reactions, indicated the presence of sulphur in considerable quantity and their relation to cystin. The colour of the urine, although of a clear yellow, had, withal, something of a greenish hue, and after standing some days a smell of sulphuretted hydrogen was given off.

The urine of the mother, of a maternal uncle, and of one adult sister, was examined without detecting a trace of cystin, but the urine of a brother twenty-four years old showed a strong reaction of sulphur, although no crystals of cystin were detected in it.

The calculus passed had an elongated shape, a yellowish colour, and waxy lustre, and its surface strewn with angular points. Its weight was 0.24 of a gramme.

The patient continued in hospital until the middle of February without any signs of illness, excepting the tremors, and occasion was taken to repeatedly examine his urine. The analytical methods are fully detailed by Dr. Trieman, but cannot be reproduced here; suffice it to note the principal results. First, a direct relation subsisted between the proportion of sulphuric acid and the cystin.

2. Urea was present in normal quantity. 3. Uric acid was considerably reduced in amount. 4. The filtered cystin-urine showed a distinct sulphur reaction, due chiefly to cystin held in solution, and when somewhat concentrated it gave off a slight sulphurous odour. The cystin was found to be discharged in the urine without interruption. The writer finds in various papers and treatises fifty-two cases of cystinuria on record; thirty-seven occurring in males, and fourteen in females. The sex is not noted in Willis's case. The disease seems most common in early and middle life. No case is on record in an individual over fifty. Details are wanting to show the duration of the affection, but it is sufficiently clear that it lasts for a considerable time, often for a period of many years. It shows no connection with occupation or position in life, or with constitutional peculiarities. The notion of Dr. Saljsbury that it is in some way connected with rheumatism has no support from statistics. The tremor of arms and hands witnessed in Dr. Trieman's case resembled that of drunkards, but cannot be assigned as a natural feature of cystinuria. A family proclivity to the disease appears to be established. In several instances it has been observed in twins, and its hereditary character has been proclaimed by some, though the foundations for the hypothesis are insufficient. Its connection with liver disease is equally unproved. The histories of cases prove that it is not always continuously excreted after once appearing in the urine, but may disappear for a time, and possibly altogether. Hence the absence of cystin in the urine is not evidence that a calculus of that substance may not exist, and, on the other hand, the presence in the urine of cystin is no proof that a calculus exists. So, likewise, the passing of a cystin calculus will not necessarily imply that a subsequent calculus has the same nature. In almost all instances the calculi have passed away through the urethra. In cases where this extrusion does not happen, and surgical interference is necessary, the right course of proceeding is by lithotripsy.

Owing to the occurrence of cystinuria without the formation of calculi, and the absence of definite symptoms and suffering with its presence, it is most probable that it is oftener present than supposed. Its rarity, as observed in women, is at least partially attributable to the greater facility with which calculi can be discharged with them.

According to Beale and Bartels, the urine discharged at night is richer in cystin than that passed by day. In some instances an inflammatory state of the urinary passages has been remarked. The hexagonal lamellar crystals vary very much in dimensions; they are also often massed together in six-sided groups. Their sides, also, are not infrequently unequal.

The treatment of cystinuria is obscure; owing to the solvent action of alkalies on cystin: the use of these agents has been largely resorted to, but their efficacy is very doubtful. As to the cause of its production in the system, no satisfactory account can be given.—*Deutsches Archives*, 1876, Band xviii, p. 232.

REPORT ON MIDWIFERY, DISEASES OF WOMEN, AND DISEASES OF CHILDREN.

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MIDWIFERY.

1. *Influence of Maternal Smallpox on Fetus.* By Mr. GEORGE RYDER ('British Med. Journal,' Feb., 1877).
2. *Conversion of Face Presentation into one of the Vertex by aid of Knee-elbow Position.* By T. H. HUMPHREY, M.D. ('Amer. Journal of Obstetrics,' Jan., 1877).
3. *Is Craniotomy, Cephalotripsy, or Cranioclasm, preferable to the Cæsarian Section in Pelves varying from one and a half to two and a half inches?* By ISAAC E. TAYLOR, M.D. Part II. (Reprinted from 'Trans. of New York Academy of Med.')
4. *Chloral Enemata in Natural Labour.* By M. POLAILLON ('Gazette Hebdom.,' Oct., 1876.)
5. *Case of Rupture of the Uterus, remarkable for its comparative causelessness so far as known, and the absence of the usual antecedents.* By ISAAC G. PORTER, M.D. ('Amer. Journ. of Obst.,' Oct., 1876).
6. *On the Thermometer as a means of Diagnosis in Pregnancy in respect of the Life of the Fetus.* By P. MARDUEL ('Archives de Toxicologie,' Dec., 1876).
7. *The Mechanism of Spontaneous Version.* By Dr. GENEUIL ('Annales de Gynécologie,' June, 1876).
8. *Sudden Effusion of Blood in the Labia of Pregnant Women.* By T. V. INGRAM, M.D. (Supplement to 'Amer. Obst. Jour.,' June, 1876).
9. *On Induction of Premature Labour by an Interrupted Current of Water.* By S. A. RABOUR, M.D. ('New York Med. Journal,' Nov.).

1. Mr. George Ryder, of Canterbury, reports six cases in which variola attacked the mothers in the later months of pregnancy. In four of these the mothers were attacked in the seventh or eighth month of pregnancy. Recovered, they went their full time, and were delivered of a living child at term; in the other two cases the patients were attacked within a few days of labour setting in, and these children were successfully vaccinated within six months after birth, and only in one child was there any integumentary disturbance, which a few days after its birth was affected with an erysipelatous eruption, but without pustule, lasting three to four weeks. This child, however, was successfully vaccinated.

2. Dr. Humphrey attended a patient, æt. 20, primipara, who had been in labour some twelve hours. The os was partly dilated and the face in mento-posterior position. There was great anterior obliquity of the uterine ovoid. After waiting two and a half hours, and the face not rotating, the patient was placed in the genu-pectoral position, and she remained in this position for some time. After waiting some two hours longer, and the chin being in the same position, conversion of the presentation into one of the vertex was decided upon, the patient being again placed in the genu-pectoral position, for she had moved in the mean time. Pillows were placed under knees, so as to bring her body to an angle of about 60° with the horizon, a hand being introduced into the uterus; the head was brought into the occipito-anterior position by pressure of the fingers on the vertex. The case then was terminated apparently normally, though this is not stated. Dr. Humphrey then goes on to say that he believes that in mento-posterior positions, where rotation takes place, or does so very slowly, and even in other face presentations, if the pelvis is not of ample proportions, it would very generally be wise to make use of this procedure, feeling confident that it would often substitute an easy natural labour for one tedious and painful, not to say dangerous, to mother and child. He also ventures to state, judging from his own case, that, by aid of gravitation conferred by the knee-elbow position, the accoucheur will be able to effect a change of presentation with comparative ease to himself and his patient.

3. Dr. Taylor draws up the following conclusions:

(1) That a mutilated fœtus can be delivered with safety to the mother through a space of one and three fourths inches in the antero-posterior and a two and a half or three inches in the transverse by craniotomy, cephalotripsy, or cranioclasm, provided the vault has been destroyed and the face made to present edgewise, or delivering the head sidewise.

(2) That after cephalotripsy or cranioclasm, if necessary, version early performed with propulsion from above the pubes afterwards, and before the uterine forces are exhausted, is preferable to the first proposition, and, I believe, more available.

(3) That the cephalotribe or cranioclast cannot be considered sufficiently available as tractors after cephalotripsy to deliver the patient in extreme contraction, and that other instruments as tractors are necessary to aid the delivery.

(4) That the Cæsarian section should not be performed when the contraction or deformity is as stated above, unless some other complications or circumstances exist or present themselves.

4. At a meeting of the Surgical Society of Paris, M. Polaillon states that as injections of chloral per rectum had been advised in normal labour for the relief of pain, he had used it seventeen times, the injections varying from two to seven grammes of chloral hydrate. In nearly all the cases, instead of alleviating the sufferings of the patient, it failed to do so, whereas the head was retarded or arrested, so that it became necessary to apply the forceps.

In a second class the chloral did not modify the labour. In a third class the labour was normal. Hence he concludes that it is useless or even hurtful to give chloral in labour, at least when attended with painful contractions of the neck of the uterus.

5. Dr. Porter was called to a patient *æt.* 39, the mother of three children, in her fourth confinement. Former labours normal. Has a tendency to obesity. On arrival slight pains had subsided. On making an examination, os high and closed. Anterior obliquity strongly marked. At ten o'clock, os being slightly dilated, the patient was placed on her back with towel around abdomen, traction was then made internally by the fingers to draw the os forward. At half-past eleven the pains became stronger, but still feeble. Dr. Porter retired to next room, when he was hastily summoned by patient's friends, who stated that she had burst in her last pain and was in great agony. He found her countenance much changed, hands and feet bathed in cold sweat. On vaginal examination the cervix was found largely ruptured, the hand readily reaching the child's head, while the body was covered by the uterus. After consultation with Dr. Mainwaring, the child was delivered by turning without difficulty. The child was dead. Placenta soon followed, the patient promised to do well, but died on the fourth day. No post-mortem seems to have been made. Dr. Barlow believes that death was due to the anterior obliquity of the uterus, causing a disproportion of the uterus, and he quotes Baudl, Chiari, Brauer, and Spaeth in support of this view. He thinks the thinning of the cervix could not have been produced by the pains, but probably existed, if it did at all, prior to labour, and he adds as a practical appreciation of the foregoing, "May we not deduce the principle that anterior obliquity may constitute in certain relations a more serious obstacle to delivery than is usually taught or believed?"

6. Dr. Marduel says one can easily see the importance of this subject as a new diagnostic sign in pregnancy, especially if the sign affords a certainty at a period when the other signs are probable. He then quotes the observations made by Cohenstein and Fehling, the latter of whom confirms the observations made by Cohnstein in eighteen cases. He then quotes Schleisinger and Klescieff, and gives the following summary:

In a gravid uterus containing a living child the temperature is from 0.27° to 0.54° Fahr. higher than in the vagina, the temperature in the vagina exceeding that of the axilla. On the other hand, in a gravid uterus containing a dead fœtus the temperature is somewhat lower than that of the vagina. The uterine temperature is also raised in case of endometritis, parametritis, &c.

He gives Alexieff's results on the temperature in the mouth and by the rectum of the fœtus, and they vary from 1.26° Fahr. to 2.34° Fahr. for the rectal temperature, and of 0.54° to 1.26° Fahr. for the temperature in the mouth above that of the vagina.

7. A woman *æt.* 28, in labour with her fourth child, was taken about midnight with slight labour-pains. On examination the pelvis was found normal. The pains becoming strong, in the middle of

next day the membranes ruptured, and the midwife who was attending recognised an abnormal presentation. Dr. Geneuil, on arrival, found the left arm, swollen and blue, hanging from the vagina, the foetal head being on the right iliac fossa, the os tightly clamping the shoulder, so that the hand could not be introduced into the uterus. Dr. Geneuil therefore judged that version was out of the question, and prepared for performing embryotomy. At the end of an hour, having made his preparations, he laid his hand on the uterus, and was astonished to find that on the left side there was strong contraction, whilst on the right there was not. As the pelvis was large he waited to see what nature would accomplish. At 4 p.m. the shoulder began gradually to recede, and by 4.30 the breech was presenting; the contractions then became uniform on both sides, and after a few strong pains the foetus was expelled dead, rather above the average size.

It is by the uterus contracting more on the one side than the other that the author believes version is accomplished in these cases, and that powerful contractions on the left side forced the breech down, whilst the laxity of right side allowed the head to recede.

8. In the first case, of which Dr. Ingram reports two, the patient, twenty-five years of age, was pregnant at the fifth month, felt a severe pain in the external genitals on being tripped up, although caught by her husband in the act of falling. On coming under Dr. Ingram's care he found a swelling the size of an orange in the left labium; this after a simple dressing somewhat contracted, then opened, and finally healed after the lapse of some time.

His second case was in a patient twenty-two years of age, at about the eighth and a half month of pregnancy. Her first indication of anything wrong was that she became aware of a lump in the right labium whilst walking. It measured about the size of a bantam's egg; the tumour enlarged and was opened after a few days, as, labour having set in, it obstructed the passage of the foetal head. Some blood and pus was evacuated. The labium healed in course of time. She gave no history of injury or anything to account for its presence.

9. Dr. Ravour first reviews the different conditions in which premature labour is indicated. He then describes three cases in which labour was induced by a current of water injected against the cervix.

The first case was a patient with a pelvis two and a half inches in the antero-posterior diameter, and nearly the same in the transverse and the oblique. She had several abortions and labours, the latter of which had either been brought to a termination by version or cranio-tomy. The labour was brought on in the eighth month, by an interrupted current of water applied to the cervix by means of a Davidson syringe. Labour-pains came on in twenty-four hours, and the mother was safely delivered without instruments. In her succeeding pregnancy a continuous current of water was kept up by an elevated reservoir; labour came on in nine days, twenty-three hours. In another case the intermittent current was again employed, labour accomplished in six days. Dr. Ravour thought the interrupted current of water gave the best results.

DISEASES OF WOMEN.

1. *Hot-water Injections in Uterine Hæmorrhage.* By Dr. WINDEBRAND (from 'Deutsche Med. Woch.,' June 17th, 1876).
2. *Extirpation of the Uterus in connection with Ovariectomy.* By GILMAN KIMBALL, M.D. ('Boston Med. and Surgical Journal').
3. *On the Removal of an Inverted Uterus by the Elastic Ligature.* By Prof. COURT (Ann. de Gynec., Sept., 1876).
4. *Absence or Non-Absence of both Uterus and Ovaries.* By Dr. A. H. GOELET ('New York Med. Journ.,' Oct., 1876).
5. *The Causation, Diagnosis, and Treatment of Utero-vaginal Fistula.* By S. LANDAU ('Archiv de Gynecologie,' Sept., 1876).
6. *Absence of Uterus, with a previous history of Chronic Inversion of this Organ, which was mistaken for Polypus and removed by Ligature, with Remarks.* By W. R. WHITEHEAD, M.D. ('Amer. Journ. of Med. Sciences,' Jan., 1877).
7. *A Case of Perforation of the Bladder by a Pessary.* By M. NOTTA ('Journal de Médecine et Chirurgie pratique,' Dec., 1876).
8. *Sub-peritoneal Fibroid Tumours of Uterus removed through an Incision in the Posterior Wall of the Vagina.* By R. STANSBURY SUTTON, M.D. ('Chicago Medical Journal and Examiner').
9. *On Vaginal Ovariectomy.* By C. E. WING, M.D. ('Boston Medical and Surgical Journal,' Nov., 1876).
10. *On Glycosuria during the Puerperal State.* By A. M. GUBLER ('Gaz. Méd. Paris').

1. Dr. Windebrand states that he first directed his attention to the subject from a paper in an American journal by Dr. Mann, where he had employed hot-water injections in two cases of abortion, whereby the pains were again aroused and hæmorrhage ceased. He relates the case of a patient to whom he was called in consultation in which ergot, ice, plugging, had been used without avail, the patient being greatly collapsed, and the cervix relaxed, due to feeble pains. Besides this he relates two cases of placenta prævia in which he had adopted the same procedure with the result of completely stopping the hæmorrhage. An injection of water of 118° to 120° Fahr. was thrown in by means of a uterine tube, which immediately caused a renewal of the pains, which, after an interval of five to ten minutes, and some eight or ten injections had been made in the mean time, ended in the expulsion of the whole of the contents of the uterus.

He found it of service also in a changed position of the organ, chronic inflammation, and in cases of hæmorrhage from fibroma; of the latter he gives a case of a lady who had two intermural fibroma, subject to great hæmorrhage on any excitement or mental disturbance. Since then he has adopted this practice in all subsequent cases where hæmorrhage is due to uterine relaxation, such as

malacostea, &c. The patient living at a distance, she was recommended hot-water injections with great success, so that she found she could now without fear undergo a long journey.

Dr. Windebrand, in conclusion, says that, if it be objected that these results obtained from the warm-water injections are not in harmony with the well-known effects of warmth, producing relaxation of tissues and dilation of vessels, we must bear in mind the fact that it is heat, not warmth, which causes a state of excitability in a contractile organ, and he regards its effects as due to stimulation of the muscular tissue, in much the same manner as cold does. As a proof of this, the finger need only to be introduced into the uterus to feel the power exerted, for as a coagulator, of course, it has no action.

2. Dr. Gilman Kimball gives the following history:—The patient was forty-eight years of age, and had been operated on eleven years before for a cystiform ovarian tumour, which after removal weighed thirty-three pounds. She made a good recovery and continued in health for six years, when her abdomen again began to enlarge, and forty-five pints of brown coffee-coloured fluid were drawn off after tapping. Three months afterwards, her health beginning to fail, Dr. Kimball proceeded to operate. He made his incision in the scar of the former one; some ascitic fluid followed. A cyst was then found and tapped, and twenty-seven ounces of coffee-coloured fluid were drawn off. On examination the pedicle was found to have embraced both the uterus and part of the broad ligament; after securing some large veins in connection with the broad ligament it was tied in two loops, which greatly diminished its bulk.

The remaining tissue was next embraced in a loop of stout annealed iron wire, and the uterus and vagina divided by two or three strokes of the knife; the point of division was about three fourths of an inch outside the iron ligature. After some delay from bleeding into the peritoneum, the vessels being secured by a carbolized catgut ligature, the pedicle was brought and secured in the mouth of the incision and seared by the actual cautery, the pedicle being too short for a clamp. The patient recovered without a bad symptom.

3. Two cases in which the inverted uterus was removed by means of the elastic ligature are reported. Dr. Courty tried to reduce the uterus, but failed. A caoutchouc tube was fastened round the neck of the uterus and its end tightly fixed by a waxed thread; it was tightened from time to time; the uterus was completely detached at the end of thirteen days. The patient was discharged cured at the end of two months. The second case was under the care of Dr. Arles ('*Paris Medical*,' Sept. 7, 1876). The ligature was applied in the same way, the uterus was detached at the end of fifteen days; there was neither hæmorrhage nor other bad result. Dr. Courty suggests that, so as more clearly to mark the line of section, a furrow should first be burned round the uterine neck by the galvano-cautery.

4. Dr. Goelet reports a case of this which he met with in a young lady of 19. She had never menstruated, complained of severe head-

aches, and suffered from epistaxis for the last five or six years. Various attempts had been made to bring on the periods, but failed.

On examination the pudendum was entirely devoid of hair; the vagina was a cul-de-sac about two inches in length. No uterus could be detected after a careful exploration of the pelvis, not even a rudimentary organ could be made out. The mammae were quite undeveloped. She had never experienced any sexual sensations.

5. Uretero-vaginal fistulae without any complications are rare. Only four cases are known, two reported by Simon, one by Alguré, and the fourth by Panas. Landau now brings forward a fifth. These fistulae are nearly always occasioned by obstetrical manœuvres, but in Landau's case it was caused by the pressure of a pessary. The symptoms of this affection are the running away of clear, transparent urine, and at the same time the voluntary passing of urine altered according to the time it has been in the bladder, the high as well as lateral situation of the fistula, and following the course of the ureter, and the possibility of passing a catheter both upwards and downwards.

Attempts to close the fistula have ordinarily failed. Simon tried paring the edges and uniting transversely, Alguré and Panas cauterised. These results were so bad that the classical works on the subject recommend as a chief and last resource closure of the vagina. Landau differs from this and points out that success may result if the two following points are attended to—the avoiding any compression of the ureter and the allowing the continual flow of the urine.

Then the vivifying of the edges should not pass beyond the superior limits of the fistula on account of the proximity of the peritoneum. Landau suggests that a catheter be passed through the lower end of the ureter into the bladder and out at the urethra, then that the catheter be passed up the upper end of the ureter, then vivify the edges of the fistula laterally and bring them together in the long axis of the ureter. If the catheter cannot be passed through the lower part of the ureter into the bladder, that part of the ureter must be incised into the bladder, and so the urethro-vesical fistula be converted into a vesico-vaginal at the upper end of which the ureter opens, then pare the edges and bring them together as above described.

6. Dr. Whitehead was consulted by a patient, a Mrs. S—, for vaginal leucorrhœa, but, on account of a statement made by the husband, he made a careful history and examination of the pelvic organs. It was shortly as follows:—She menstruated at twelve, and was married at thirteen. Shortly after marriage her husband was killed. Six years afterwards she married again, and aborted at the fourth month. She got up within three days. A month afterwards she lost blood, which continued more or less for four years, being attended by numerous physicians. Afterwards she came under the care of Dr. Bancroft, and Dr. Justice furnished Dr. Whitehead with the following particulars. They diagnosed an intra-uterine fibroid with broad pedicle, which they removed by constricting its neck by means of a

silver and a double canula. The fibroid sloughed and came away in pieces after a few days; he further stated that a meddlesome nurse pulled at the ligature and inverted the uterus; subsequently another physician placed a loop or ligature round the tumour and strangulated it, and then something came away. From that time she ceased to menstruate, but had leucorrhœa.

Dr. Whitehead found on vaginal examination the os patulous, but the cervix enlarged and indurated. By rectal examination and by a catheter passed into the bladder, the body of the uterus was found to be absent and only a slight induration where the junction of neck and body should be. Dr. Whitehead then afterwards adds, "An interesting question may arise regarding the mode of the mechanism of inversion. How far will pressure on the fundus during labour, to facilitate expulsion of the child, aid in causing inversion?" He had always spoken favorably of the plan during manual and instrumental delivery. He had a case with inversion lately following the expulsion of the child by pressure on the fundus, and coupled with a very short cord was the cause of the inversion; the uterus was immediately and readily reduced; ergot was given to prevent its recurrence, and the patient placed in the genu-pectoral position for an hour before he felt certain the inversion would not be reproduced.

7. The patient in this case had long worn a winged pessary for prolapsus uteri. After a violent exertion she was suddenly seized with a violent pain in the abdomen and incontinence of urine. On examination it was found that one of the wings had perforated the bladder; the wound healed so little that afterwards the operation for fistula had to be performed. M. Notta also cited two other cases, in the first of which a pessary used by a quack caused abscesses and fistula, and in a second death resulted.

8. This is the first case related in which a uterine fibroid was removed through the post-vaginal walls.

The patient, according to Dr. Sutton, was a coloured woman, æt. 50, with a large solid tumour, ovoid in shape, which filled half the pelvic cavity, pushing the uterus to the left. He thus describes the operation:—Ether being administered and the bladder emptied, the posterior wall of the vagina was seized midway between the uterus and perinæum by a tenaculum, and cut through with one stroke of the scissors. By a probe-pointed bistoury and the aid of a tenaculum this incision was extended as far as possible towards the rectum and cervix; bleeding was arrested by cold water; the peritoneum was picked up by a Sims' small tenaculum and cut with scissors. This incision was made the same as the former, by a probe-pointed bistoury. An attempt was made to enucleate the tumour by the fingers, but it failed; then with the hand previously dipped into carbolized water, the tumour was grasped forcibly and its adhesions broken down. It was found to be attached by a fleshy pedicle to posterior wall of uterus. By means of vulsellum passed over the tumour it was drawn into the vagina, the intestines, which had at first come down, being carefully replaced. Another incision was then made in the exposed capsule through a speculum. After re-

peated incisions of the capsule and dragging of the tumour by the aid of guarded hooks the growth was brought out of the pelvis. Two vessels in the pedicle were ligatured. The pedicle, with some folds of small intestine in the vagina, was now pushed back into the abdominal cavity; no stitches were applied to vaginal wall.

The patient died some six hours after the operation. Post-mortem examination showed a wound in the small intestine made by the prong of the vulsellum. The wound was the size of a pin-hole, besides which was a rent completely glued together. Doubtless, the escape of gas, he says, was one of the causes of the peritonitis.

9. The patient a lady, whom Dr. Wing saw with Dr. Lowell, on February 10th, 1876. She gave the following history:—She was thirty-two years old, unmarried. Menstruation always painful; at the age of twenty, after lifting a heavy weight, she began to suffer pains in the back, worse during the periods, since when she had used morphia, and sometimes etherization during menstruation. Five years ago the uterus was retroverted; a year later Dr. Storer felt an ovary in the Douglas's pouch. Since then she had kept to her room. She suffered with constipation, frequent micturition, poor appetite, but no febrile symptoms. Per vaginam, an elastic swelling in Douglas's pouch, pushing the uterus forwards, fundus backwards, but movable with sound, the latter entered normal depth. Under ether the tumour could be pressed up. Per rectum, the mass pressed against the sacrum.

The tumour was aspirated per vaginam; a small amount of dark bloody fluid was withdrawn. From the nature of the fluid it was deemed to be an old hæmorrhagic effusion. On March 30th aspirator was again used. Same fluid was evacuated; after this she suffered from a slight form of septicæmia. Wing operated in much the same way as the case reported above by Dr. Sutton, by drawing the posterior wall down by tenaculum, incising the vagina, and then tapping the tumour. There was no distinct pedicle, but the uterus, tipping backwards, allowed the broad ligament and Fallopian tube to come into view. Dr. Wing thought of tying this, but Dr. Warner contrived to enucleate it; there was some bleeding, but no ligature was used. Dr. Wing inserted three sutures, so as to prevent hernia of intestines. The patient did well. The subsequent history of the case calls for no remark.

10. Dr. Gubler in a note which he communicated to the Biological Society of Paris, states that a transitory glycosuria may occur in pregnancy. His conclusions are—

(1). Glycosuria is not a normal phenomenon of the state of lactation.

(2). It shows itself on the suspension or premature suspension of lactation, provided the nurse be in good health and not affected by any serious constitutional disturbance.

(3). In other terms, glycosuria only appears as a consequence of a rupture of the equilibrium between the production and consumption, giving rise at first to a lactosemia comparable to a super-albuminosed blood from which is derived the albuminous dyscrasia.

He has never seen transitory albuminuria accompany glycosuria in these pathological conditions.

DISEASES OF CHILDREN.

1. *On Chronic Pneumonia of the Apex in Children.* By S. FLEISCHMANN ('Wiener Medicin Press,' Dec. 20th, 1876).
2. *On Two Attacks of Scarlatina in the same Patient.* By H. HUTTENBREMNER ('Jahrbuch für Kinderheilkunde,' Band x, heft 3-4).
3. *Cold Bathing in the Treatment of the Summer Diarrhoeal Affections of Infants.* By CORNELIUS COMEGY, M.D. ('Amer. Jour. of Med. Science,' Jan., 1877).
4. *Hereditary Transmission of Syphilis.* By M. KASSOWITZ, M.D., of Vienna ('Stricker's Jahrbücher').
5. *On the Treatment of Umbilical Hernia in Children.* By M. ARCHAMBAULT ('Paris Medical,' Oct. 12th).
6. *On Chloral in Infantile Convulsions.* By LOWENSTAU ('Medicinisch Churgi. Centralblatt').

1. (1) Unilateral swelling of the lymphatic glands of the throat, of the back of neck, of the submaxillary region, when other local causes, such as pharyngitis, parotitis, alveolar inflammation and diphtheria, can be excluded, causes strong suspicions that there is pneumonia of the apex of the same side. The glandular swelling continues whilst the process in the lung is active, and cases when the lung infiltrations become stationary, the glands swelling and subsiding with each advance of the inflammation.

(2). Certain obstinate forms of conjunctivitis which, in spite of treatment and without apparent cause, return from time to time with great severity, if but one and always the same eye be attacked, point with great probability to disease of the lung of the same side.

(3). Eczema of one half of the face or head, which heals with difficulty and frequently occurs, sometimes alternating with or accompanied by ophthalmia of the same side, should lead to examination of the lung, where pneumonia of the apex of the same side is often present.

(4). Certain sympathetic disturbances of the one side of the face or head, having frequent changes in colour from flushing to pallor, transitory circumscribed erythema of the cheek or temple, always on the same side with the pneumonia, the easy production of Trousseau's maculæ, which also accompany meningitis, cerebral tumours and other diseases being excluded, often indicate pneumonia of the apex of the same.

(5). Intermittent sympathetic neurosis affecting one side of the head, characterised by redness and elevation of the temperature of the affected side, is often observed in children with lung infiltration of the same side.

(6). Finally, neuralgia of the trigeminus, oculo-motor and vagus

nerves occurred and disappeared during the process in the lung of the same side in such a manner that no certain relation between the two could be determined. These symptoms were observed in so many cases that the author regards them as reliable.

2. Two cases are recorded. The younger of two brothers, aged three and a half and seven respectively, had a well-marked attack of scarlatina followed by lamellar desquamation. He was four weeks ill. The brother returned after the rooms had been thoroughly disinfected. At the end of six weeks he took the disease. As the brother had recovered only two months before, no precautions were taken. After twelve days he became feverish, had vomiting, and complained of difficulty of swallowing, the throat was red. On the next day he had a rash of greater intensity than the first, and after fourteen days he desquamated. There are grounds for believing in a family predisposition in this case, for the mother suffered during each attack of the younger with sore throat, diphtheritic patches, with high and transient and early albuminuria and hæmatoma, followed the first time by brawny desquamation, and the second time by acute rheumatism.

3. The treatment of diarrhœa attended with fever, says Dr. Comegy, has been so successful with me, that I am confident if applied more generally it would lessen very greatly the rates of mortality in the summer season. I allude to that form of disease which is denominated enterocolitis.

Before we are called to these cases tentative measures for the relief of the diarrhœa have been already applied by the friends, so that the inflammatory stage is generally fully developed when we see the patient. The skin is hot ($T. 102\frac{1}{2}^{\circ}$ to 105°); the pulse rapid, 130 to 150; respiration 30 to 50, with frequent purging of semi-fluid, greenish, watery, fæcal, and half-digested matters; the mouth and tongue are dry, the thirst intense, but the water taken to slake it is quickly thrown off; the eyes are staring, pupils contracted, insomnia, and rolling of the head, with uttering of distressing cries, due to headache from hyperæmia of cerebral vessels and unappeased thirst. Such is a general statement of symptoms.

I at once proceed to give the little sufferer a bath in hydrant water, which with us in summer is about 75° . I have found it necessary to give this my personal attention at first, because the mother's friends will not carry out the instructions on account of the child's cries. The contact of a hot skin with cold water is certainly painful for the moment; hence I immerse the body from legs upwards, gradually sponging the skin in advance, so as to obtain tolerance. When the body and extremities are covered I pour water over the head for about ten or fifteen minutes. Meanwhile the child ceases to cry or struggle, and is evidently greatly comforted; it drinks water with avidity, owing to its thirst.

The child is then wrapped unwiped in a woollen shawl, and put to bed; the pulse has lost frequency, but is quite feeble; the breathing is slower and the skin quite cool, even bluish in hue. The sedation at first seems too great, but reaction soon begins as healthy warm

perspiration is established, and the child falls into a peaceful sleep.

The scene has so changed that one will find no difficulty in having it repeated three or four times in the twenty-four hours if the alarming symptoms again supervene ; these may be required for two or three days. In the mean time internal remedies should be given. Quinine, whiskey, beef tea, milk, and lime water, are the chief agents. One grain of quinia and a drachm of whiskey every four hours for a child eight to sixteen months old looks rather formidable, but they will be found admirable when the disposition to fever lasts. Subsequently bismuth and pepsine are of great value to restrain drowsiness and to assist digestion, so greatly at fault owing to the blow which the mucous membrane has suffered.

4. Dr. Kassowitz gives in the reports of the Hospital for Children in Vienna his valuable experience. In the first place he refers to the direct transmission by the sperm or germ cell infected by a syphilitic father or mother ; and in the next to infection in utero from syphilitic mothers.

The depth and seriousness of the affection is by the first method, and involves the embryo from its generation ; this fact is supported by the author's own observations. Out of 119 cases he finds the inheritance was clearly from the fathers in 43 cases, the mothers being healthy in that number ; both parents were syphilitic in 23 cases, the mothers alone in 10 cases ; in the remaining 43 cases the mode of inheritance was doubtful. Thus, the father is most frequently the source of infection, and this bears out the view that a healthy mother may give birth to a syphilitic child. He believes this to be of frequent occurrence. He does not think that a child who is syphilitic from the father can infect the mother through the placenta. Similarly, syphilis acquired by the mother during pregnancy is not transmitted to the child. Syphilis is only transmitted by fathers or mothers at the period of conception. He notes that the intensity of the virus diminishes in direct arithmetical proportion to the lapse of time. The first child after a recent infection will die in utero at the fifth or sixth month, the next will live till the seventh or eighth month, then a child may be born alive at full time, but with an eruption of pemphigus, or the eruption may begin during the first days after birth, or in later cases not till the second or third month. If the first occurrence of the eruption is delayed to the third month the intensity is already much diminished in the parents. It is exceptional for a child to be born alive during the earlier years after infection of the parents, unless mercurial treatment has been used. It may be transmitted with the entire absence of symptoms, and the intensity of the symptoms in the child seems entirely independent of the presence of its manifestation in the parent.

5. Dr. Archambault, having experienced the difficulty of keeping an umbilical bandage in its place, has devised the following plan. He takes a piece of white wax, which he softens and rolls between his

fingers; the ball is then divided in two halves; one of these, suitable to the size of the hernia, is placed in the depression and retained there by means of strapping: gutta percha previously softened by hot water does equally well. In an hour or two both of these will be found adherent to the skin; after this no bandage need be applied, a cure may be expected in the course of a month or two.

6. Dr. Löwenstaum having found chloral very useful in diseases of children with convulsions, gives a case as an example in detail. The patient was the third child of a woman highly nervous, who lost her second and third child with the same symptoms as in the one recorded. At the thirteenth day twitchings of the eyelids and corners of the mouth were noticed, these increased until general convulsions were set up, which repeated themselves every ten minutes. On being called to the child, he observed it had twitchings of the face, trismus, clonic spasms of the limbs, spastic contraction of the thumb, and contracted pupils; the fit ended by profuse sweating in about five minutes. Two grains of chloral were given every hour, and the convulsions at once lessened, so that by the following day they had entirely disappeared. The cause was thought to be dyspepsia; an antacid of magnesia water was given and there was no recurrence of any convulsion.

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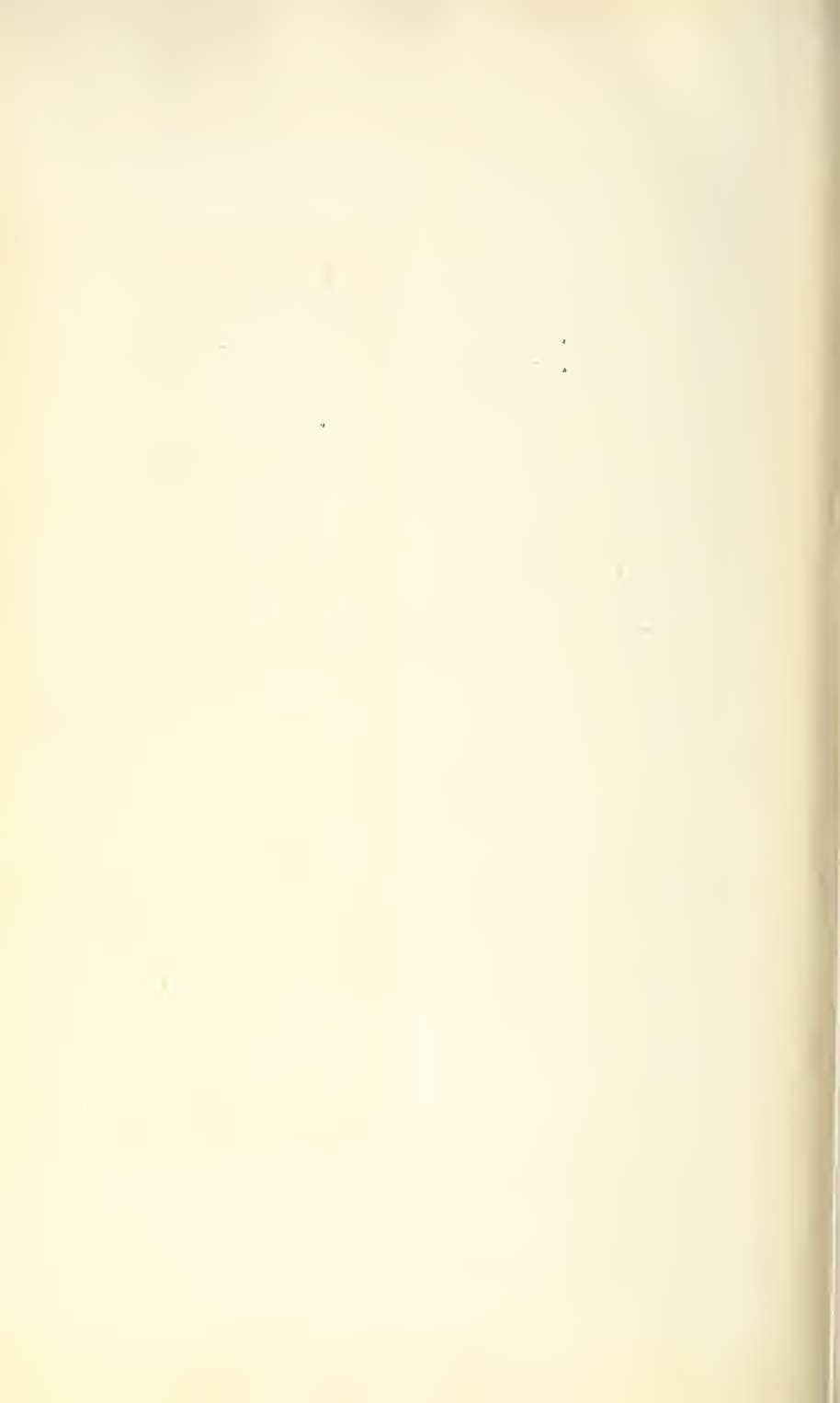
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